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Miscellanées d'ancient arabe XVI

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Foreword

This XVIth volume of the series entitled Miscellanées d'ancient arabe which started in 1971 is composed of ten chapters dealing with NA and SA inscriptions, viz., respectively, IV, VI, VIII, the second half of both V and IX and I, II, III, VII, X, the half of both V and IX.

Although the shortest of all chapters, IV with two new Tham inscriptions is nonetheless of primary importance because of the origin of the findings, viz. the southeastern section of the Ar peninsula. With the exception of that chapter IV, the balance of the volume is devoted to the review of past publications. It can safely be said that the numerous pages devoted to SA not only prove abundantly the complete failure of the 1982 SDDS in all the instances where it can be called upon but also confirm unequivocally (cf., e.g., A.F.L. Beeston's and J. Ryckmans' nonsensical elucubrations on Qat ʿrm in pp. 92fol., and C. Robin's use of the same text to prove two antinomic theses in pp. 72 and 74) the veracity of my last assessment of the work of the laudemus invicem club members, which reads as follows in JaMAR, XIV, p. 303:

"If, however, the reader has the patience to read all those lines or any section he chooses and, therefore, to follow step by step the development of the aberrations, distorting arguments, downright irrational and/or nonsensical theories or opinions of those authors as well as their systematic vindictive partisanship, I am certain that he too will feel, as I do, nothing but loathe for those authors' work and sorrow for the important setback incurred by SA research."

After presenting the showcase of SA research offered by four authors on RÉS 4839, chapter I ends up with two appendices the first of which is a systematic checkup of the references listed in p. 69 of BeSG. Chapters II, III and IX review three recent books, viz. the second volume of CIASA in II, Sayhadica in III, and A Cemetery at Queen Alia International Airport in IX, where W.Rollig's and E.A. Knauf's epigraphical contributions are particularly offensive. The remaining chapters are devoted to papers by D.F. Graf in V, U. Hübner and Knauf in VI, and especially on several papers written by W.J. Jobling in VIII, which shows them to be the work of an author who, like Graf, has less than an embryonic knowledge of the problems involved. Of special importance is chapter VII which is devoted to the vindication of Mrs. Mabel Virginia Anna Bent for being unwarrantably accused of an archaeological fraud by R.L. Cleveland. Finally, chapter X brings to light and exposes W.W. Müller's partisanship and distortion of the written word.

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I - RÉS 4839, a showcase of SA research.

RÉS 4839 is yet another example of the ugly state of SA research, and this type of publication is condoned and implicitely approved by scholars whose apparent goal is to apply for their own benefit the basic principle of the laudemus invicem club members and, in the process, they set aside the only purpose of research, the objective truth, provided that, of course, they know what truth is. RÉS 4839 is only one manifestation , among many, of the climate of selfish flattery which has long since taken over SA research and is increasing year after year so much so that SA research must still brace itself for a long, long period of obscurantism or voluntary intellectual jahiliya. Many other similar examples of a research distorted by selfish considerations and aberrant argumentations were denounced in my review of SDDS published in JaMAR, XIV, pp. 62-269 (cf. also below, X).

RÉS 4839 is a yellowish libation altar published as Min by E.Mittwoch and H. Schlobies in Orientalia, 7 (1938), pp. 350-352,and was republished in 1950 by RÉS (VII, p. 386) in RÉS 4839 as Ḥaḍr on the sole basis of "t" of "b^{3}mrt par son ordre" at the end of the inscription.

A - In 1962, JaSIMB (p. 178A) republished again the epigraphical documentation of the altar on the basis of my study of the original itself, viz. text A above the lateral edges, and text B on the front side of the plaque; the former "b^{3}mrt" is erroneous instead of b^{3}mrh upon his[= of the author of the text] order; therefore, the altar is presented as Sab, as I had already stated in 1957 in my volume entitled La paléographie sud-arabe de J. Pirenne, Washington, pp. 108-109.

B - The same year, but independently from JaSIMB, A.F.L. Beeston deals with the text in his own distorted way in Oriens, 15 (1962), pp. 454-455.

1 - Beeston does not refer at all to my 1957 statement on the stone.

2 - RÉS's testimony, "qu'il a (dédié à?) ... Dhū-Ḥanim par son ordre," is distorted in Beeston's prose to become "by His (the God dḤNm's) order" and "the name of the masculine deity dḤNm." In RÉS's translation, the idea of offering is a mere hypothesis suggested by the author without any support in the text, and in this mere conjecture, dḥnm could at the very most be an appellative of an unknown name of deity, and certainly not the name of the deity itself.

3 - Then Beeston states that "in the photograph the bottom part of the

last letter is not visible: consequently it could be y just as well as t" (p. 454). He does not even see that the two upper bars of the fork are vertical; therefore, regardless of the bottom of the letter, it is h. But, "in the photograph" actually is a mere excuse for Beeston and his like, which is used as a pseudo-scientific screen protecting their so-called scientific remarks. The above-described condition of the two upper bars as well as the straight lower extremity of the lower bar are clear on my squeeze made in August 1956.

4 - Beeston's additional proof, at least in his mind, that RÉS's reading and translation of the last word are erroneous is that "to read it [= the last letter] as t neglects the fact that the hadrami masculine pronoun-affix is -s, while the affix which is spelled indifferently ś or t is feminine. This point does not seem to have been observed previously, but a careful examination of the instances will show that ś/t is preceded somewhere in the context by a feminine antecedent" (p. 454). This evaluation Beeston had already published it in BeDGSA in 1962, p. 8 in 3:8: "The chief characteristics of HAD are: ... masculine singular pronominal affix s^{1} or s^{1}ww ..., but feminine t or s^{3}" (cf. also, pp. 45-46). Beeston's preceding statement on the Ḥaḍr pronoun is erroneous as shall be demonstrated below.

5 - Finally, in order to prove that the provenance of the piece "is in fact the Jof" (p. 454), Beeston emphatically states that "the person from whom Rathjens purchased the piece explicitely mentioned the Jof as its provenance." The only information C. Rathjens gives is "angeblich aus Sūda im Djōf" (cf. Die unlokálisierten Funde, Hamburg, 1955, p. 277, photos 559-560). Rathjens was more realistic and knew better than Beeston. It is indeed a well-known fact that the information given by natives is not always accurate. Furthermore, Min inscriptions are not the only ones found in al-Jawf.

C - In AION, 33 (1973), p. 34, footnote 7, G. Garbini reaffirms the Min character of RÉS 4839 and repeats BeDGSA's statement on the Ḥaḍr pronouns. Of course, nobody expects Garbini to refer to JaSIMB's original study of the antiquity.

D - In 1974, A. Capuzzi publishes her Iscrizioni minee (Naples), a publication loaded with all kinds of errors and characterized by a complete lack of critical judgment. She publishes (p. 114, no. 387) her worthless reading "("Lettura: in base alla foto")" which contains no less than five errors without counting the four dots representing four inexistent letters. Of course, JaSIMB's decipherment from the stone itself, including that of text B on the front side which even the editors mention but did not completely decipher (RÉS never refers to text B), is valueless

for Capuzzi who is doubtlessly protecting herself behind Garbini's name, although she occasionally acknowledges such a dependence (e.g., pp. 6, 9, 10, etc.).

E - In 1976, JaCMYE discusses the Ḥaḍr personal pronoun (pp. 187-190) and states that the masculine form is -s and t, -sww" (p. 187) and that "-t of bnt [of RÉS 2640/2] is missing in BeDGSA (pp. 45-46 in 37:6); this is a deliberate omission ... The omission is probably the result of the author's wish to establish a parallel between Ḥaḍr and Shauri" (p. 188).

F - In 1982, in his BeSG (p. 69,par H 23:2), Beeston maintains BeDGSA's position but adds his remarks on RÉS 4839 and 2640 in footnote 130.

1 - For my remarks on BeSG, cf. JaMAR, XIV, pp. 126-132.

2 - RÉS 4839.

a - Beeston never refers to JaSIMB's reading of the two texts A and B. Therefore, Capuzzi is in good company with both Garbini and Beeston.

b - Beeston states that [A] "its[=of the stone] probable provenance is the Jawf; [B] hence it is classed as Min in M 387."

Ad [A] - The certainty of origin stated emphatically by Beeston in 1962 gave place, without any reference to the first, to a mere probability.As mentioned above, such a probability is not even justified based as it is on Rathjens' "angeblich."

Ad [B] - Beeston's "hence" also is a distorted presentation of the facts. Capuzzi could not have ascribed the antiquity to Min because of its origin since she clearly states this: "Provenienza sconosciuta" (cf. l.c.). She classified it as Min because Beeston himself and Garbini did so. Therefore, Beeston's "hence" is a vicious circle.

c - The best proof that Beeston had completely lost his 1962 emphatic assurance is that he states immediately after his statement "In any event" which proves that he really does not know which way to go.

d - Beeston adds that "the text is to a considerable extent illegible." Here again, Beeston's ugly haughtiness is manifest: he simply sweeps under the rug without any justification or even any mention of it JaSIMB's reading which, as already stated above, was based on the study of the original and contains the two texts. Beeston does not really care about a decipherment from the original and, if and when he accepts it, he may even change it to suit his own whims.

e - The grand finale of Beeston's argumentation is this:

"its b³mrt, if taken as meaning 'by his command', would be anomalous whether in Min or in Had." This is Beeston at his best, viz. confusion, contradiction and error. Since he affirms "its b³mrt," he in fact admits it to be the correct reading. Then, what is the justification of his "anomalous"?; how does he translate the expression?; what does he do with his 1962 reading of "b³mry"?;how can he speak of Min?;and how can he reject Ḥaḍr?

3 - RÉS 2640.

In an apparent answer, without saying it, to JaCMYE's reproach that his BeDGSA had not mentioned bnt of RÉS 2640/2, BeSG adds at the end of his footnote 130 the following statement: "In R 2640/2 bnt could be understood as 'part of it (the town, ḥgr, fem)' rather than 'his son'," an interpretation which, according to C. Robin (cf. Syria, 64 [1987], p. 19), "paraît moins vraisemblable." Here again,Beeston's argumenting method is at its best.

a - Beeston does not even attempt to justifiy his translation of bn as "part." The only justification he has is that he wants at all costs -t of bnt to be feminine.

b - ṣdqyd^{c} manifestly is a personal name and cannot be that of a section of a town.

c - Beeston's reference of -t to "the town, ḥgr" is erroneous. The author of the text states that qtdm/mḥny/gn³/wyf^{c}t/whlft/ - - - /

wmḥny/³byt/g^{c}l/³hy/gn³t

he was in charge of the building of the enclosure wall of Mayfa^{c}at and of its [= gn³'s] gate

and

of the building of the houses he [= the author] had made next to its wall.

³hy: BeSG (p. 70, par H33:3): "? beside?" comes from N. Rhodokanakis' "neben" upon reference to Ge^{c}ez "ḥayya:³anta ḥayya" (cf. Studien zur Lexikographie und Grammatik des Altṻdarabischen, I, Vienna, 1915, pp. 59-0; cf. also RŚS, V, pp. 15-16) = "per, praeter, trans eum (illum) locum" (cf. A. Dillmann, Lexicon linguae aethiopicae, New York, 1955, col. 13). C. Robin distorts the meaning of the word when translating ³hy as "contre(?)" in Syria, 64 (1987), p. 19 and the archaeologists on p. 15.

The end of the text reads as follows: wwd³/w^{c}ly/dgn³/bnt/ṣdqyd^{c} and he [= the author] finished and completed to the top what his son Ṣadiqyada^{c} had walled in.

Here again, Beeston carefully avoids giving his own translation of the whole text. All that he does is to throw in his translation of one small section and leave to the reader to figure out how it fits in the whole text.

Contrary to BeSG's position on -t being exclusively feminine (p. 69, par H 23:2; cf. also C. Robin in Syria, l.c., p. 19), -t is singular masculine in RÉS 2693/2-3 (cf. already RÉS, V, p. 16): -/sqny/- -/sqnyt/dhbhn/dmdlwt/hlfm/dhbm/qyhm/

w

qṣ²t/dt/šft/-

In dmdlwt, d refers to dhbhn and introduces a substantive parenthetical clause composed of mdlw, hlfm, dhbm and qyhm, and -t obviously refers to the preceding d, a well-known feature (e.g., dšfthw in Ja 582/6). The verb qṣ² is the continuation of the main proposition after the incidental clause and parallels sqny; therefore, its complement refers to that of sqny, viz. sqnyt (cf. also BeDGSA, p. 46, par 37:6).

It should be strongly emphasized that in neither of his so-called grammars, Beeston mentions even once the case of dmdlwt although the text is utilized 5 (five) times in BeDGSA. In BeSG (p. 69), Beeston refers twice to RÉS 2693, in par H 31:6 and 32:4. The first reference is not listed in p. 72, under "R 2693." In the second reference, Beeston assigns the conjunction kmw to l. "1," which must be corrected to "4." The author also writes this in his first reference: "One example of asyndetic coordination may perhaps be found in R 2693/1 ²dn/qny/mlkn 'retainer (and) slave of the king'."

1 - The "and" should be placed between brackets, not parentheses. The restitution of the conjunction w is unjustified as the text can easily be understood as it is, and Beeston does not even attempt to justify his restoration. The expression ²dn/qny/mlkn is perfectly paralleled by, e.g.,
tly/²frs/mlkn (Ja 584/1) the equerry of the horses of the king, and
²tlwt/²frs/mlkn (Ja 745/2-3) the equerries of the horses of the king.

2 - Beeston's translation of the pericope is irrational. How could a "slave," who has nothing, possesses nothing, not even his life, order (he has no right whatsoever) and pay for (he has no money at all) a bronze plaque and a bronze statue apparently of sizable dimensions since the weight and the quality of its metal are given in the text? How could a "slave" be a "retainer" of a king since he already belongs to him? A "retainer" is nothing but a servant; the rank of the person he serves does not in the least affect his basic social status, and a slave cannot possibly be a servant.

3 - The translation of ²dn as "retainer" (cf. also SDDS, p. 2) is erroneous.

The Ar root ²dn implies the idea of "listen; allow;" cf. ²adina "to listen to s.o." and "to allow s.o." (cf. DAFA, I, pp. 70-73). Taking the contexts into consideration, the word ²dn may be referred either to ²adin "porter, usher, chamberlin" or to ma²dun "person authorized to (negociate; ...)" (cf. DAFA, l.c.). The noun may be translated here as "deputy, appointy."

4 - For qny, normally a collective, but also a singular, as "slave(s)," cf. already JaSIMB, p. 447B.

The expression ²dn/qny/mlkn may be translated the deputy of [in charge of] the king's slaves.

Appendix I: Remarks on BeSG's p. 69.

Here follow my remarks on BeSG's references expressly given in the same page 69.

A - In par H 20:3, Beeston's copy of CaTh 4/5 mistreats the name of the month as "ds¹mwy," which must be corrected to smw. There is no y in either G. Ryckmans' reading or the photo (cf. G. Caton Thompson, The Tombs and Moon Temple of Hureidha (Hadhramaut), Oxford, 1944, respectively, pp. 158-160 and pl. 63, 3).

B - In par H 25:1, Beeston states that "a form dhy which seems to be mas du in AM 758/1, 3." Cf. CIASA, I, pp. 159-163. The reference to l. "1" must be deleted; it is erroneous.

Beeston's understanding of the text (cf. below) is manifestly erroneous because he has to consider dhy as referring only to the two sons of the author, leaving the latter hanging in the thin air. Such a construction is disproved by all SA material.

Beeston actually ignores or forgets the following epigraphical facts: the four verbs of the text (ll. 3-4, 9, 12 and 13), all of which having the authors as subjects, are in the plural; and the only personal pronoun used in the text is the plural masculine -sm, which is repeated no less than six times. This epigraphical evidence is so self-evident that it proves without the shadow of a doubt that dhy is not at all a "mas du," but a manifest masculine plural, as it has three antecedents. For the text itself, cf. below, p. 13.

Beeston writes in his p. 68, par H 12:4 that "the construct du termination is ... possibly -hy in a few instances, e.g., R 2689/3 gs¹mhy/gn²/qlt 'the two sections of the wall of Q' (i.e. those on each side of the gateway)" (cf. also p. 69, note 31; C. Robin endorses Beeston's theory in Syria, l.c., p. 19). First, "2689" must be corrected to 2687. Secondly, the parenthesis contains nothing but his explanation of his understanding of -hy, which remains unjustified.

The engraver of the text says that he was in charge for his lord, a Ḥaḍr mukarrib, of hbn²/ᶜqbthn/qlt/wᶜqb/ḥgr/ᶜlhtyhn/bn/bḫrhn/ - (3) -/wgzmhy/gn²/qlt/wᶜqbhn.
N. Rhodokanakis' translation, which is reproduced in RÉS (V, p. 128), p. 39, reads as follows: "zum Bau der Feste ḲLT und die Festen, welche die zwei Höhen vom Meere absperren ... und (bei) der massiven Aufführung der Mauer (der Feste) ḲLT und der (anderen) Festen."
This translation needing some improvement, J. Ryckmans wanted to provide it by giving his own interpretation of l. 2, which reads as follows: "pour munir d'un mur la passe (ᶜaqaba) de qlt, et les passages du Ḥajer: les deux hauteurs (qui le séparent) de la mer" (cf. BiOr, 11 [1954], p. 137B; H. von Wissmann published his German rendering of Ryckmans' translation in Zur Geschichte und Landeskunde von Alt-Südarabien, Vienna, 1964, p. 199). The following remarks are in order.

a - Here, Ḥaḍr h- is a preposition, not a conjunction, as in "pour munir." Note that DoSA also takes h- and bn² as a conjunction and a verb, "to build," respectively. DoSA's translation follows Rhodokanakis' except for the two preceding words (p. 181).

b - The noun bn², the reading of which is ascertained in DoSA's photo 79 (p. 154) means "building," not at all "munir d'un mur," which is the meaning of gn².

c - ᶜqbt, pl. ᶜqb, means "hill" (not a "passe") and, therefore, as here, a fortress atop a hill or promontory. That ᶜqbt is located southeast of the main wall which blocks the access to the interland.

d - Ryckmans' restoration remains unjustified because it has no counterpart in the text. Furthermore, his "le" refers to "mur;" then, his statement makes no sense because the two prominences are not between the wall and the sea.

Beeston's understanding of the gender of gzmhy of RÉS 2687/5 as a dual is made very clear by his parenthesis, "(those on each side of the gateway)." But, the fact that the stone bearing the text is located in the defensive bastion of the wall containing the gateway has nothing to do with the interpretation of gzm. The stone is inserted in the gateway because it is the best place for all persons passing through the gate to see and read. In fact, DoSA's photo 79 shows a serrated wall composed of many sections the design of each of which is well adapted to the ground ruggedness to give the wall the maximum strength possible, just as it is at Sumhurâm (cf. AlAAE, p. 14 and chart in pl. 4). Furthermore, gzmhy is determined by both gn²/qlt and ᶜqbhn and, in turn, these ᶜqbhn refer to both ᶜqbthn/qlt

and ᶜqb/ḥgr. It is precisely to all those sections of the fortresses and the wall that the word gzm refers; thus, gzmhy is a construct plural, an interpretation which is also endorsed by BeSC in the same p. 68, in par H 13:2, 3 end.

The above-mentioned pericope may be translated as follows: for the building of the fortress Qalat and of the fortresses of Ḥagar, the two prominences off the sea ... and (for the building) of the sections of both the wall of Qalat and the fortresses.

Two more remarks on C. Robin's study of RÉS 2640 in Syria, La. C.

a - Robin considers the possibility of gᶜl[end of the line]²hy being a single word. But, the nouns of the qtl² form are extremely rare; cf., e.g., ẓ²d² of Ja 750/7; and the hapax legomenon ²hy is reasonably explained by Ceᶜex. Robin's remark is purely semantic because such a possibility is simply dropped in the rest of his study.

b - Robin translates dgn²/bnt/ṣdqyfᶜ as "ce que l'enfant(?) de celle-ci (= Myfᶜt), Ṣdqyfᶜ, avait (déjà) muni d'une enceinte" (p. 19). In an effort to prove that -t refers to myfᶜt, Robin refers to three collective expressions where bn or bnt are related to the name of a town. But, such an expression is never used in the singular. Furthermore, the addition of "déjà," which should have been put between brackets, is superfluous even in his interpretation. Finally, on ṣdqyfᶜ, cf. above, p.5, the remark in 3b.

C - In par H 25:1, Beeston also adds this: "R 2687/5 and CT 10/3 dhy introduces an eponym dating and its morphological analysis is dubious (see par H 30:1)." It is most interesting to compare the preceding statement with BeDGSA, p. 67, 57:8, which reads as follows:

"A HAD enclitic -hy ... appears to be reliably attested by CT 10/2 bhy/ᶜddm 'in (the year of) ᶜddm'. On the basis of this, in the closely parallel passage R 2687/5 the Répertoire's dhy should evidently be read (b)hy."

BeSC's texts must be corrected in four places.

a - CaTh "10/3" and "10/2" must be corrected to 10/2 and 10/2-3.

b - CaTh 10/2 has bhy which is clear on the photo of the edition, and is correctly read by G. Ryckmans (cf. G. Caton Thompson, The Tombs..., p. 64, 1, and p. 162, respectively). The compound preposition dhy of RÉS 2687 is clear on the photo published by DoSA, p. 154, no. 80.

c - Here again, Beeston does not justify his position, viz. his rejection of G. Ryckmans' explanation of bhy in the commentary of CaTh 10/2, which reads as follows: "l'élément d'indétermination ḥaḍramoutique hy, en minéen ²y" (p. 162).

d - For Beeston's "eponym dating," cf. the end of the next "Appendix."

D - In par H 33:1, Beeston refers to dlsm = d+l+sm to "R 3512/2." His "2" must be corrected to "1." For the text A and B of RÉS 3512, cf. JaMAR, II, pp. 63-64.

Appendix II: Note on CaTh 10/2-3.

G. Ryckmans reads the text as "bhy/ʿddm/slâhn la troisième (année de l'éponymat de) ʿAdîdum" (pp. 162-163); the commentary is on p. 162 and reads as follows: "Ici on lirait plutôt 𐩣𐩣𐩣, mais le trait horizontal qui caractérise le 𐩣 dans la présente inscription, pourrait n'être qu'un renforcement de la ligne horizontale qui marque la section médiane des lettres pour faciliter le tracé au lapicide. Cette ligne est également accentuée au début de la l. 2."

The conditional mood of the first phrase is incorrect: the reading of wddm is certain. The suggestion of the second phrase is unacceptable because it does not explain either why the length of that additional bar exactly equals the width of the circle and why the width of the tracing of the bar also is absolutely identical with that of the tracing of the circle. Finally, the third statement is erroneous: the center of b of kbr is marked by a circular feature of the stone which cannot be misrepresented as a horizontal line.

Ryckmans' argumentation was apparently suggested by the parallel of RÉS 2687/5 which he transcribes as follows: "bhy/yšrḥ²l/dʿddm." But, his "bhy" must be corrected to dhy. It seems obvious even at first sight that the family name ʿddm of this text cannot prove that it is a personal name in CaTh 10/2.

Ryckmans' idea of "éponymat" was endorsed by BeSG (cf. above, p. 9, C). Yet, there is nothing in the text which might indicate that the individual was an eponym. On the contrary, since the text deals with the extensive repair of the facade of a temple, it seems much more normal to think of that individual as the main priest in charge of the temple, and the repair would have happened during his third priestly service (cf. also below, IX, B).

The name of CaTh 10/2-3 is wddm Waddam. For wddm, a qttl (reduplication) nominal derivation, cf. Sab wdm in Ja 2774 ap, and Min wdmn in Ja 2352 a, and Ar wadam "redundantia, augmentum" (cf. FrLAL, IV, p. 453A).

II - Review of CIASA, II.

A - Introduction.

CIASA, II, follows its predecessor by nine years. Apart from an obvious and no doubt earthshaking difference between them, which is visible at the very start, viz.I is called "tome" and each part a "section," but II becomes a "volume," and each part a "fascicule," II is written in the same vein and has the same value as I. It would, of course, be tedious to repeat my criticisms of I published in JaCIASA, pp. 260-278; they still stand and, as could have been expected, CIASA has done nothing to correct them; they are still repeated in II.

The subtitle of II is "Le Musée d'Aden." I find it incredible that J. Pirenne's "table des matières" does not even give the list of all the NAM (= National Aden Museum) sigla dealt with in the volume; of course, she prefers the sigla of CIASA classification. It is also incredible that she does not give the list of the concordance of the former AM and the NAM sigla. Even, in pp. X-XI, she lists 13 texts + Ja 122 republished in I. Since all AM numbers have been changed by, and because of, Pirenne herself to become different NAM sigla, it would have been necessary as well as very easy to give right there, in pp. X-XI, the new NAM sigla those antiquities have received. Apparently, such a logical attitude was too much for Pirenne, infatuated as she is by her own messy sigla system of CIASA. At least, however, the reader became aware that Folkard 1, which was published in I, pp. 139-145, did not reach the final destination it was claimed by Pirenne to have. She, indeed, wrote in I (p. 139) that "lors de mon séjour à Aden en décembre 1961, la pierre était au domicile de M. Doe, destinée au Musée d'Aden." Maybe D. B. Doe will make known what he did with that antiquity! Furthermore, the same list repeats the illogical attitude already seen in I,pp. VI-IX, where the siglum she herself gives is turned around and is read backward; cf. p. X: "47.12/s4/P46" was published in I, p. 437 as "P46/s4/47.12," and on p. XI, "95.11/s4/P44" was also published in I, p. 421, as "P44/s4/95.11." Therefore, the reader may safely think that for Pirenne there is no difference between backward and forward.

Pirenne also states that "je refis tout l'inventaire [of the museum] en at-

tribuant à chaque pièce un numéro provisoire, selon un classement typologique préétabli et exprimé par des numéros codés, avec une marque distinctive pour les faux" (p. III).

On the one hand, if her classification is "provisoire," why was it done at all? What is the purpose of it? Was it a change for the sake of changing? When is the definitive classification going to take place?

On the other hand, if her classification was "typologique", how is the reader to understand, e.g., that the three statuettes belonging to the same type (pp. 283-286), as she herself admits since she lists them under the unique label of "Trois statuettes-stèles, masculines" (p. 283), could have been given sigla as apart as 1726, 1255 and 39? Such is not an isolated case; others are, e.g., the five "statues sans pieds ni socle" (pp. 295-297) also have sigla as far apart as 2625, 599, 1888, 1883 and 76; the two "bases de statues de rois de ʾAwsān" (pp. 317-320) with sigla as far apart as 621 and 1302.

The most disheartening of Pirenne's statements reads as follows: "les pièces restantes devant trouver place dans un ou deux autres volumes qui seront, je l'espère, préparés par mes jeunes élèves yéménites, devenus diplômés des Universités de Paris" (p. III). The quality and value of Pirenne's work are well-known, and more of the same is found in the following pages. More apropos to the present case, she herself provides the reader with two outstanding examples of her guiding skill in pages 209 and 211, which may be treated briefly here.

NAM 2340 is a very short Qat text published in pp. 209-210 by "Hamūd As-saqqaf Supervised by J. Pirenne December 1981" (p. 209).

The -nyhn emphatic dual ending of l. 3 is commented upon as follows: "cp. ṣlmnyhn in Ja 342/2, which is considered as an emphatic dual ending in the Qat. dialect" with reference to BeDGSA, "par 29:10" on p. 33. But, the -nyhn ending had already been known for a long time prior to that reference:

in Min, e.g., hgrnyhn of RÉS 3316/6;
in Qat, e.g., mqmnyhn of RÉS 3566/10, and even
in Sab šʿbnyhn of RÉS 4196/1.

That dual ending was already so well-known that even M. Höfner did not make any remark on it in her publication of the last text referred to above more than fifty years ago in WZKM, 40 (1933), p. 25.

NAM 188 is another short text published in pp. 211-212 by "Hamūd As-saqqaf supervised by J. Pirtenne [sic!] November 1980" (p. 211), as follows:

Transcription	Translation
1) ʿBʿMR/ḌSRHT	1) ʿBʿMR, of (the clan) SRHT,
2) HQNY/ʿBDY/ʿLMQ H	2) has dedicated two slaves to ʿAlmaqah.

The word divider at the end of l. 1 and the number "3" are missing in the transliteration. - The d of l. 1 does not necessarily introduces a "clan" name; srḥt can be either a personal name or a family name. - The expression of ll. 2-3 could hardly be easier than it is. G.Ryckmans translated it in RÉS 4967 (cf. RÉS, VII [1950], p. 457) as "a dédié les deux esclaves de ʾAlmaqah" and justifies his translation in p. 458. Even if Pirenne does not wish to read the comment of G. Ryckmans, to whom she owes everything in her career, she should at least have enough good common sense not to leave isolated a construct form which is by nature determined by the following word or pronoun. The translation is also erroneous because ʿbd means "servant," not "slave" which is rendered by qny.

My review of CIASA, II, is composed of the study of five inscribed antiquities, viz. RÉS 3902, no. 120, 4829, NAM 224, 483 and 2753. As a fitting introduction is added here the study of two more inscribed antiquities found in I, viz. AM 758 and RÉS 3965. All those texts were studied at the occasion of the preceding section of this volume; therefore, they were not deliberately selected.

B - Qat AM 758 (cf. Pirenne, in CIASA, I, pp. 159-163).

Pirenne reads the text as follows (pp. 159-160):

Transcription	Traduction
1) ṮWYBM/WBNSW	1) Ṯuwaybum et ses fils
2) W/MRṮDʿM/WʿB	2) Marṯad'am et ʿAb-
3) KRB/ḌHY/WDM/S	3) karib, ceux (du clan) de Waddum,
4) QNYW/MRʿSM/WR	4) ont consacré à leur seigneur Wara-
5) FW/ʿMRʿM/ṢLMM	5) fû de l'oracle de ʿAm une statue
6) ḌHBM/HWFYSM	6) d'or parce qu'il les a protégés
7) WWFY/BYTSM/ʿ	7) et qu'il a protégé leur maison, leurs
8) ḤRRSM/WʿDMS	8) hommes libres et leurs serfs.
9) M/RṮDW/GRWBS	9) Ils ont confié leurs champs aménagés à
10) M/WRFW/MRṮDM/W	10) Warafû, en bien confié et
11) ʿḌNM/ḌYNʿM/HS	11) en propriété, (ce) qui amènera prospérité pour
12) M/WKL/ḌQNYW/BḌ	12) eux et tout ce qu'ils possèdent dans
13) T/ĠYLM/WSRṮYW	13) ḏât-Ġaylum; et ils (Le) rendront compatissant.

"It is clear," according to A.F.L. Beeston, "on the basis of the occurrence of the enclitic HY in line 3" "that this text is ḥaḍramitic in dialect" (p. 163). What Beeston has in mind is that the relative pronoun dhy, being attested in the Ḥaḍr texts but an hapax legomenon in Qat, cannot be understood as a Ḥaḍr import into that Qat inscription, as is the preposition h- in hwfysm (l. 6) and hsm (ll. 11-12). Were the pronoun dhy frequently attested in Ḥaḍr, Beeston's reasoning would

be acceptable. But, such is not the case: the pronoun is extremely rare even in Ḥaḍr. What reason does Beeston have to dismiss that the same could not be the case in Qat as well? The presence of the hapax legomenon dhy in AM 758, which is massively Qat because of the word grwb and the names of persons, place and deities, does not, and cannot, by itself prove that it is a Ḥaḍr import regardless of the preposition h-. Beeston had apparently no difficulty accepting the preposition l in Ḥaḍr (cf. BeSG, p. 69, par H 33:1) although it is an hapax legomenon in RÉS 3512 B/1, which he erroneously transcribes as "R 3512/2." The enclitic -hy is indeed used also as the plural construct ending

in Min, either masculine (e.g., mrḍwhy/ʿttr of Ja 403 the favorites of ʿAttar),

or feminine (e.g., hlfhy/hgrn of TaAM 6/1 the gates of the city),

in Qat, either masculine (e.g., ʾhhysm of RÉS 3691/3 their brothers),

or feminine (e.g., nfshysm of Ja 118/3 their roof-terraces).

Ll. 2-3: ʾbkrb/dhy/wdm: it should be noted that a ʾbkrb/dwdm is mentioned in Ja 928/4, a text from al-ʿUqlah.

Ll. 4-5 - On the Qat god wrfw, Pirenne refers to A. van den Branden's paper in BiOr, 16 (1959), pp. 183A-187A. Of course, "1859" of Pirenne's commentary is a printing error instead of "1959." The referred-to paper is so poor that even the gender of mnḍh - mnḍhy - mnḍht is all mixed up. Pirenne adds that mnḍh "n'est d'ailleurs à prendre que comme un qualificatif: 'protecteur' et non pas comme un nom propre de divinités." Not only mnḍh is a singular, disproving the plural of "divinités," but also her theory is contradicted by all the texts where mnḍh is used as an individual deity having its own temples; e.g., mnḍhhmw/bʿl/ s̄ʿbn of CIH 41/3. Pirenne then refers to A.F.L. Beeston's note (cf. Oriens, 1 [1962], p. 48) which is merely devoted to "the identification of wrfw as a mnḍh," which is rejected, and Beeston suggests retaining "wrfw as a deus terminus." This is a redundancy: latin "Terminus" already means the god of boundaries. The rejection of the identification of wrfw and mnḍh was nothing new; it had already been repudiated in 1947 by JaP (pp. 124-127), to which he, of course, pays no attention. Then, after stating that "the identification of wrfw as a mnḍh rests solely on Rhodokanakis's restoration of the lacuna in R 3965," viz. "mnḍhs[m/w]rfw (l. 5), he states that "it would be equally possible to restore mnḍhs[m/ww]rfw or simply mnḍhs[/ww]rfw." These two suggestions were already erroneous at the time Beeston wrote his paper because only two letters were supposed to be missing, not three (against his first suggestion), and the verb br² and the only personal pronoun

used in the text are in the plural, viz. br²w (l. 2) and -sm (l. 7) (against his second suggestion) (cf. below G, the study of Ja 3198/7).

L. 5 - ʾmrʿm means order/command of ʿAmm. Pirenne's translation of ʾmr as "oracle" (cf. already SDDS: "sign; omen; oracle" in p. 6) is erroneous because "oracle" does not necessarily imply an order, a command, which is the "notion de base [of ʾmr]: ordre, commandement" (cf. DAFA, I, p. 202A; cf. also M. Cohen, Dictionnaire des racines sémitiques, Paris, 1 [1970], pp. 23B-24A). As JaP (p. 125) states: "il était naturel de placer sous la protection du dieu-terme les abords de ce lieu," viz. the place where ʿAmm made his orders known.

L. 6: hwfysm is translated as "parce qu'il les a protégés." Pirenne writes that that "le H initial est l'équivalent ḥaḍramoutique de la préposition 'pour'." Indeed, she translates hsm (ll. 11-12) as "pour eux." But, by some unexpected illogical fancy, she translates here the same Ḥaḍr preposition as the conjunction which indicates the reason of the action and, in so doing, she transposes to the past the future of the request. Note also that her "il" is an error instead of "Il" as in l. 7; cf. also her "Le" in l. 13.

Ll. 7-9: bytsm/ʾḥrrsm/wʾdmsm is translated as "leur maison ,leurs hommes libres et leurs serfs." The word ʾdm means "servants," not "serfs," a word which implies that the person and his goods depend on a master. SDDS (p. 2) translates the word as "vassals, subjects," thus equating two basically different social conditions. The two words ʾḥrrsm/wʾdmsm are an explicative apposition to bytsm as they are not introduced by the conjunction w and. Thus, the word ʾḥrr refers to the relatives or friends of the authors who might live with them, and the word ʾdm, which serves as the plural of ʿbd (cf. already above, p. 12), refers to their servants.

L. 9: rtd: Pirenne's translation of the verb as "confier" is misleading because such a verb implies that the depositary alone is holding or has access to that which the depositor entrusted to him. In SA, the situation is different: the general care, protection, safety and well-being of things, both humans and non-humans, are entrusted to the deity while the proprietor does not relinquish his normal, daily use of that which is entrusted to the edity. — grwb: the meaning of "terraced plots" is suggested by the antinomy of the expression ʾsrrs/wgrwbs of RÉS 3856/1, viz. his lowlands and his terraced plots. LaGD's opinion, "la traduction Feldterrasse [of N. Rhodokanakis] est, d'après moi, erronée et ne peut être motivée que par la configuration du terrain dans les montagnes" (cf. I, p. 276).

This opinion is strictly based on Ar and does not take into account the context of RÉS 3856/1. It is upon LaGD's testimony that Pirenne translates grwb as "champs aménagés" as if the lowlands were not "aménagés." Pirenne's last remark is to point out that "on notera que le mot GRB a, dans certains contextes, le sens de 'corps, personne' mais le pluriel semble en être GRYBT." She missed the mark: grb as person; body is Sab, and its plural is grbt and (more frequently) grybt. The idea of person in similar Qat contexts is expressed by nfs.

Ll. 10-11: the two apposed substantives mrtdm/w²dnm indicate the kind of rtd the authors had in mind. Pirenne's translation of ²dn as "propriété" remains unjustified: not even a single word of her commentary is devoted to it (p. 162). The noun ²dn subjection is often used in Qat in the expression b²dn + a divine name (for J. Ryckmans' erroneous theory on b²dn/ᶜm, cf. JaMAR, XIV, p. 188). It would be irrealistic to translate ²dn as authority as if the deity had either ordered all those texts, including those mentioning only the short pedigree of the author, or covered the act of engraving by its autority. The word signifies the author's recognition of his own subjection to the deity. — dynᶜm: "(ce) qui amènera prospérité." In all probability, her "(ce)" refers to the act of rtd. Such a construction is grammatically unacceptable; d must refer to ²dnm, viz. the expected effect of ²dn is ynᶜm; cf. Ar naᶜama, 2nd form "commodam et iucundam vitam paravit" (cf. FrLAL, IV, p. 304).

L. 12: dqnyw: Pirenne translates a perfect tense as a present, "ils possèdent."

Furthermore, her translation considers kl/dqnyw as depending from the preposition b- of bsm. But, since the authors of the text live in the city dt/ġylm Dāt-Ġaylum (ll. 12-13), presently Ḥajr bin Ḥumeid, dqnyw all that they have owned there has to be real estate and slaves. As nᶜmt always is requested for themselves or other living human beings by the authors of the texts (cf., e.g., RÉS 3914 /3 and TC 993+1261+1001/2), kl/dqnyw cannot depend on b- of bsm, but must depend on rtd as its second direct complement, with mrtdm/- -/bsm as a long parenthetical clause. Furthermore, kl/- -/ġylm could not have been inserted before mrtdm/- -/bsm because buildings cannot provide with a happy life in the future more than in the past.

L. 13: srtyw: Pirenne translates it as "ils (Le) rendront compatissant." But, she restores an inexistent personal pronoun to give some meaning to her otherwise unintelligible phrase, and understands a perfect tense as a future, thus reversing her error in l. 6 where she had translated a future request as a past event. For srty, Pirenne refers to LaGD's "ratiya li- plaindre, avoir pitié de, compatir à" (cf. II, p. 1126). The meaning of "avoir pitié de (li-)" is also attested in the Near-East modern Ar (cf. C. Denizeau, Dictionnaire des parlers arabes de Syrie, Liban et Palestine, Paris, 1960, p. 192). Here, the 4th form is in the passive voice.

The text of AM 758 may be translated as follows:

1 Tuwaybum and his sons,
2 Martadᶜamm and ²Ab-
3 karib, they of the (clan) Waddum, have
4 dedicated to their lord Warā-
5 fū of the Command of ᶜAmm, a male statue
6 in bronze for their safety
7 and the safety of their house:
8 their freemen and their servants.
9 And they have entrusted their terraced plots
10 to the care of Warāfū in trust and
11 subjection which will provide them with a happy life,
12 and [also] all that they have owned in Dāt-
13 Ġaylum. And they were shown pity.

The deity's positive response tu human pleas was so far expressed in the texts by the word ḥmd (cf., e.g., Hamilton 7/2). Here, the authors voice the same idea in a different way, "they were shown pity."

C - RÉS 3965.

Some details of my remarks are based on my later squeeze of the original. Pirenne's treatment of the antiquity in CIASA, I, pp. 117-123 is dealt with in the following lines.

1 - Origin.

Pirenne first refers to M. Höfner's catalog of the Glaser material, viz. Die Sammlung Eduard Glaser, Vienna, 1944, pp. 30 and 91, which states that, according to Glaser, the antiquity comes from "Kohlān," not "Kuhlān-Timnaᶜ," as Pirenne affirms. Seven lines further down, Pirenne states that "On m'a dit qu'elle venait de Kuhlān-Timnaᶜ" (p. 117). What is the purpose or advantage of repeating exactly M. Höfner's information unless she implies that she actually knows the precise origin of the antiquity independently from Glaser? But then, who is "on"? Such a statement is very strange. According to her own testimony (p. 117), she photographed the stone in "1961." On the assumption that the unidentified "on" gave her that information in 1961, how can she reasonably expect anyone to tell her from

personal experience 67 (sixty-seven) years after the fourth and last trip of Glaser in 1892-1894 (cf. E. Glaser, Altjemenische Nachrichten, München, [1908] 1910, pp. 8A and 11A; and E. Komorzynski in Österreichische Lehrerzeitung, 9 [1955], p. 84A: "Die vierte Reise (Jänner - Frühjar 1894)," and the same author repeats the same information in Bustan, 1964, p. 69B) that the original came from "Kuḫlān-Timna'"? Furthermore, Pirenne erroneously speaks of "l'estampage fait par Glaser." Glaser never made the squeeze himself. Even A. Grohmann states that "Schon E. Glaser i.J. 1894 Abklatsche von qatabānischer Inschriften aus ... Ḥağar Koḫlān ... beschafft" (my underlining) (cf. Arabien, München, 1963, p. 105). Glaser had no first-hand knowledge of the location of the stone and, even were that information correct, the stone being reused in a modern building could have been brought from the wadi.

2 - Dimensions of the stone.

Pirenne's "58x45" for the stone is manifestly erroneous. The accurate dimensions are given in JaMAR, III, p. 75, a publication which Pirenne ignores, where the dimensions of the lines, which are not given by Pirenne, are also mentioned. The only information that JaMAR forgot to give is the interlinear space; it is 0.7 cm.

3 - Decipherment.

L. 2: "S³[" of Pirenne, which is not accounted for in her translation, must be corrected to either ⌜ʾ⌝[or ⌜s⌝[. There is no second letter. - "HNW" of Pirenne must be corrected to (b)nw. The lower half of the initial letter is destroyed in such a way that it is impossible to determine whether the letter was b or d. Of course, the context excludes the second reading; nonetheless, there is just not enough of the letter on the stone to read it as b.

L.8: add a word divider between bnw and ʿhr.

4 - Identification of the authors.

Pirenne devotes 2/3 of her p. 118 to her restoration of l.1 on the basis of Folkard 1 published a few pages further in the book (pp. 139-145; cf. above, the remark on p. 11). Her argumentation is a succession of four gross errors.

a - Preliminary note.

Folkard 1 is republished by M. Höfner in Sayhadica, pp. 39-45 and photo in p. 48B.

b - Remarks on Pirenne's study.

(1) Pirenne speaks of "l'identité des graphies" (p. 118); cf. also, p. 123: "les mêmes caractères stylistiques que

ceux" de Folkard 1. This evaluation is a gross error: the style of the latter is slenderer and more elaborate than that of RÉS 3965. The most characteristic difference is the lower half of ṯ. That section of ṯ in RÉS 3965 is a bottomless rectangle (1.5 wide and 1.7 high) composed of three straight lines with two rounded angles. In Folkard 1, it has the form of a bell with well-rounded curves.

(2) Pirenne speaks of "la qualité de ces personnages qui sont ici des chefs de clan" (p. 118). The only information given by the text is bnw/tʿym (l.2). It takes Pirenne's wild imagination to even think of "chefs" in such an expression. Furthermore, she cannot prove that tʿym is not a human being, the head of the family. It is in an effort to make her dream more palatable that she adds "des chefs de clan, comme devaient l'être les bénéficiaires du décret royal" de Folkard 1. The reader also notes that the hypothesis of the status of the persons mentioned in Folkard 1 (cf. "devaient être") becomes a reality, a certified fact on p. 139, where she characterizes Folkard 1 as being written "en faveur d'un clan."

(3) Pirenne's third error is to eliminate an essential part of the testimony of Folkard 1. This text does not even give the family name or clan affiliation of the recipients of the royal decree. That she does "restituer leur clan: banū Taʿym" (p. 139) is a mere conclusion of her initial hypothesis and is an unfounded invention. In fact, the absence of any family name or clan affiliation in Folkard 1 makes her initial hypothesis practically inexistent and shows that, here again, it is a mere preconcsived theory forced upon the texts.

(4) Pirenne's fourth error is her restoration of RÉS 3965/1 on the basis of Folkard 1 as "ʾḫrm/wd[rʾm/bnwy/sʿ]dm" (p. 118). Although fitting the gap very well, Pirenne's restoration contradicts Folkard 1 because ʾḫrm and d[rʾm] would be the sons of [sʿ]dm, but in Folkard 1, ʾḫrm, drʾm, ʾḫdb and sʿdm are listed as if they were brothers, and they lived in different houses, ʾbyt, contrasting with [byt] of RÉS 3965/3. Such a list of four names cannot be understood as the names of the three brothers followed by the name of the father of those three men. Pirenne continues her argumentation adding that "d'autres personnages du même clan, et leurs deux fils sont cités; ce devaient être des cousins." Note first that "d'autres" actually mean two, viz. ʾlnʾd and ʾbsʿd. Pirenne's speculation is contradicted by [byts]m [the]ir [house] (l.3) in the singular, which is the natural restoration already suggested by Rhodokanakis because there is no room to restore the plural "ʾbyt." If those persons were "cous-

ins," they would live in their own houses or in their father's. Moreover, in the hypothesis of -smy of bnsmy referring to ʾln²d and ʾbs⁶d, the dual should indicate these two persons to be a couple. The most apparent understanding of ll. 1-2 is as follows: ʾḫrm/d[r²²1/
w²lf]dm/w²ln²d/w²ls⁶d
whnsmy/(²) or (s)[.../w.../]-
bnw/t⁶ym
ʾḫrm and d[... are husband and wife; then come the husband's three brothers, and the couple's (-smy) sons; and finally, t⁶ym is the name of their father and grandfather.

(5) The only difficulty of the text is stb⁶ of l. 3. N. Rhodokanakis bases his translation of the verb as "festfügen" on the Ar expression ṯabaʿa ʿamalahu "he made his work sound, or free from defect" (cf. LaAEL, p. 2498). The preceding interpretation is unacceptable because it gratuitously gives the so-called 4th verbal form the meaning of the 1st. Pirenne invokes the Ar expression ṯabaʿa l-bāriʾ l-qawsa, which presents no interest in the case and its mention is all the more useless in that it has no effect on her opinion. On the basis of the 4th form of Ar tabiʿa, viz. ʾatbaʿa which means "made to follow: send after; place after, put behind" (cf. SaAAL, p. 63B), Pirenne formulates her opinion as follows: "Ne s'agit-il pas seulement de faire élaborer l'édifice, étage après étage, avec soin, faire monter l'édifice?" But, what else could any builder do but to build "étage après étage" ou "faire monter l'édifice"? Therefore, what is the justification for such a truism? I would like to suggest an entirely different interpretation, viz. stb⁶ as the 8th form of sb⁶; cf. Ar sabʿa "to make sevenfold, or of seven," cf. the expression sabaʿa l-ḥabla "he made the rope, or cord, of seven strands" (cf. LaAEL, p. 1296A) and the fact that the 8th form also has one of the meanings of the 1st form, "to steal" (cf. LaAEL, p. 1296A and C). This etymology is supported by Ja 118/2-3 and 119/2-3, where the description of the house Yafaʿ easily accounts for seven sections. Indeed, among the six substantives describing the sections of the house four are in the plural (ʾḫtb, mqft, nfshy and rfdhy, the latter only in Ja 118) and another one is in the dual (ṣrḥty); and noteworthy of a special remark is that no attention is paid to the actual number of stories.

The pericope br²w/wswtr/wstb⁶/[byts]n may be translated as follows: have constructed and put the foundation of, and made of seven sections, [th]eir [house].

(6) The expression bn/ʿrsn/⁶d/fr⁶n from foundation to top is very well-known (e.g., RÉS 3552/3-4) composed of two perfectly symmetrical halves and indicating the totality of the building in the vertical direction" (cf. A. Jamme in R. LeBaron Bowen, Jr., et al., Archaeological Discoveries in South Arabia, Baltimore, MD, 1958, p. 187A). Here again, Pirenne sought to improve the translation of that expression and states that "nous gardons le sens de l'adjectif arabe, imagé et précis: ṣaris 'dur, inégal, rude (terrain, endroit)' etc." and, consequently, she translates the expression as "depuis le sol brut jusqu'au faîte" (p. 120). But, the text does not speak of ground; the word ʾrḍ is nowhere to be seen. On the contrary, the text speaks of the building of a house, and both substantives ʿrs and fr⁶ are parts of the house, not of the ground. Furthermore, Pirenne is so blinded by her own idea that she does not even see how ridiculous it would be to build a house upon "le sol brut" because her expression "le sol brut" cannot mean "ground level" but must refer to the ground unaltered by human activity. Moreover, she does not see either that she practically eliminates from her reasoning the verb swtr, which she herself translates as "ont fondé" and which refers to the building of the foundation of the house, and the foundation of a building belongs as much to that building as any other section.

D = RÉS 4829 and NAM 2753.

RÉS 4829 = HambVG 31.300:1980a:rectangular amulet; 2.2 x 0.9 cm; thickness: 0.1 cm. Cf. the photo in E. Mittwoch and H. Schlobies, in Orientalia, 7 (1938), p. 234; my tracing is on the right.

The text of the amulet was republished in JaSIME, (p. 103) as follows:

ḫgr/⁶zyn/w	1	Ḫagar [to] ʿUzayyin; and
ltn⁶ly/r	2	that She may be exalted for having
f²n/hw/b	3	healed him (Ḫagar) from
mhbs²m	4	mistreatment inflicted by a malefactor.

In CIASA (II, pp. 275-277), J. Pirenne publishes NAM 2753, an alabaster "buste de la déesse ʿUzayyan." My detailed review, JaCIASA (pp. 260-289, and in particular, p. 267), denounces "la hantise de J. Pirenne de statues de dieux et déesses." The present publication adds many examples, including NAM 2753, to those of vol. I of the same erroneous theory. NAM 2753 is the bust of a nude woman holding her offering between her arms, and was most probably deposited in a tem-

ple. An inscription is engraved on the back of the body; this fact alone already proves that the text is spurious (cf., e.g. Ja 444), and to confirm it, l.2 has an unintelligible "hnrhgr." Pirenne is not quite ready to accept the preceding facts, and she finally states that "l'inscription est bonne si l'on admet que le R (de RNSRGR) est une erreur pour L" (p. 276). It would have been very easy for Pirenne to add "premier" between "le" and "R," but it is difficult to expect exactness from her. Unfortunately, she cannot prove her hypothesis, and the simple fact that her suggestion is invented for the specific purpose of identifying the goddess ʿayn with the female bust already proves its deliberate preconception. Furthermore, Pirenne's analysis leads her to the impossible compound divine name ḥgr/ʿayn, although she refers to "le dieu Ḥagar...;HGR apparaissant aussi comme épithète de ʿAṯtar et aussi dans le nom divin Ḥagar Qahamum" (to be corrected to Qahawum). She does not even mention the opposition between masculine ḥgr and feminine ʿayn (cf. JaP, pp. 96-97 and 106-107, respectively). Who knows? Pirenne might have received a revelation from either deity on the androgynism of either one. At the occasion of her brilliant study, she refers to RES 4829/1, "où le verbe prouve qu'il s'agit d'une déesse" (p. 276). As if the reader did not understand the impossibility of her divine compound name, Pirenne makes very sure to tell him that ʿayn is a goddess.

At Pirenne's apparent invitation, A.F.L. Beeston obligingly intervenes and declares "untenable" the interpretation of tnʿly as the 7th form of ʿly, as given in my translation, because

[A] "such stems do not exist in any Sabidic language. It is obvious that we [B] have to supply a word-divider after WLTN, hence:

[C] «Protection of al-ʿUzza and Lat be upon R.»

Omission of a word-divider is fairly common, specially in the less elaborate texts."[D](p.277).

Ad [A] - Such a reason is completely valueless even for Beeston himself for he should know very well how many hapax legomena he inserted in his SMDS of which JaNAR, XIV, pp. 62-269, wrote a lenghty, detailed review (cf. also below, X). What he should have done is to prove that the verbal form tnʿly was morphologically impossible. Finally, for Beeston's novelty of "Sabidic," cf. below, III A.

Ad [B] - What is the justification of his "obvious"? What is "obvious" is - as usual - his determination to justify any of his concoctions at the expense of what the engraver wrote.

Ad [C] - "The blinding power of Winnett's preconceived theory ... had to be devastatingly effective to prevent him from seeing how preposterous his statement really is." This phrase of JaNAR, XV, p. 126, also applies to Beeston in the present case: he translates "wltn" as "and Lat," thus sweeping under the rug of his haughtiness the letter n. What he really had in mind is not, as he says, "to supply a word-divider," but to correct the annoying n to a word divider; thus, "wlt/" instead of "WLTN/." Furthermore, in flagrant contradiction with his [A], Beeston carefully avoids informing his reader that he now invents without any justification the mention of NA lt in a Sa text.

Ad [D] - The adjective "fairly common" is another invention by Beeston for the sake of the cause. His statement is nothing but an empty babbling of which he does not even give a single reference. Nobody has the right to invent a word divider unless the text cannot be understood otherwise. Furthermore, if the engraver wrote a word divider between rfʾn and hw where it clearly is superfluous, how can Beeston explain that the same engraver would omit it in wltnʿly where it would be most necessary ... according to his phantasy?

His translation of ḥgr as "protection" is one of those hapax legomena of his SMDS (p. 67) which mentions the verb ḥgr and the substantive mḥgr(t) with the identical basic meaning of "reserve," viz. "to reserve, be reserved/land reserved." Without any explanation, the meaning of ḥgr is altered for the sake of the present text and becomes "protection" as if "reservation" were synonymal of "protection." The preposition associated with Ar ḥajara is ʿly, and ḥajara ʿalā means "to prevent, hinder from (ʿly)" (cf. LaAL, p. 516). SMDS (p. 67) translates lthgrn/ʿly of "Ln 3/1" (to be corrected to 3/3) as "be reserved for (ʿly) s. o." This interpretation of ʿly is excluded by the same SMDS, p. 17; the basic meaning of ʿly is "against." SMDS's translation of Ln 3/3 is unacceptable: the text deals with hlt the coffin of a lady. A translation according to which that coffin of woman would be "reserved for" her children is unthinkable. For the translation of Ln 3, cf. JaCNIE, p. 124.

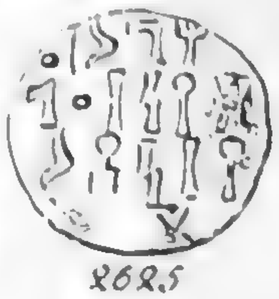
Furthermore, Beeston, followed by his SMDS, does not give a clue as to where he picked up his translation. In fact, he carefully avoided referring to CIH 559, and it is CIH's (II, p. 332) translation of this text that Beeston and his SMDS did copy. CIH reads the text as follows:

ḥgr/ʿ	1	"Protectio ʿU-	y/ngr	3	per Nagre-
ayn/ʿl	2	zayyi su-	m	4	s."

At least, J.C. Biella mentions the preceding text which she translates as "the consecration of (the goddess) U. (is) over N. = N. is consecrated to U.' (on an amulet" (cf. Dictionary of Old South Arabic. Sabaean Dialect, Chico, 1982, pp.

166-167). The wording of the translation is awkward because of the unspecified, unusual meaning given to the word "consecration," and the following phrase given as synonymal of the first expresses a reality which cannot be considered as the equivalent of the first and even less as the rendering of the original text. CIH 559 may be translated as follows:

1 Hagar [to] ʿU- 2 sayyin a- 3 gainst Nagr- 4 um.



It should be noted that the palaeography of the letter r in both CIH 559 (cf. the facsimile published by J.H. Mordtmann in Himjarische Inschriften und Altertümer in den königlichen Museen zu Berlin, Berlin, 1893, pl. 1,no. 2625) and RÉS 4829 is identical, and this suggests that the two texts are the work of the same Hagar. The circles and circumferences of RÉS 4829 do not alter the palaeographical type of the letters.

A repulsive aspect of Beeston's work is this: by isolating ll. 1-2 from ll. 3-4, he does not shrink from offering his reader a stupid translation. His "R." cannot but be the initial letter of at least a proper name; but Beeston does not even bother to inform his reader whether he considers it as the name of a person or of a place. As mentioned above, ll. 2-4 are read as follows: rf³n/hw/bmhb³sm. What does Beeston do with the personal pronoun which is not even listed in his SDDS (p. 55)? How can he translate b of bmhb³sm except as "from, away from," a meaning which the same SDDS excludes from b (p. 24: "in, at, with, by") but acknowledges only in bn (p. 29)? The same total opposition between the prepositions b and bn is also spelled out in no-doubtful terms by Beeston in BSOAS, 35 (1972), p. 353, where it is the sole basis of his argument. He writes that "she [= the female dedicator of Ja 751] was kept alive not 'from' (bn) the hry but 'in' (b) it" (cf. already JaMAR, XIV, p. 227). If the reader puts together the information given by both Beeston and his SDDS, he will necessarily get the following translation:

1 "Protection of al-ʿUzza and
2 Let be upon R"
3 F³N him in (OR at, with, by) (cf. SDDS, p. 24)
4 a person causing damage. (cf. SDDS, p. 25).

Is that not a stupid translation? The letter n is eliminated; the personal pronoun hw necessarily refers to the immediately preceding personal name (the only one in his whole text) ... unless, of course, Beeston considers rf³n/hw as one of those rare names, such as ³mt/³bh of CIH 533/1, pretexting - why not? - that the worker put a word divider in the wrong line, viz. in l. 3 instead of l. 2; and "protection" is requested upon something or somebody ... "with" or "by" a malefactor!

Pirenne ends up her study of MAM 2753 by adding two phrases as her magistral appendix to Beeston's comment on RÉS 4829. She writes this:

"(Pour WTN «protection», voir *Sabaic Dictionary*, 1982)
On voit que c'est une déesse protectrice. " (p. 277).

Pirenne apparently did not realize that Beeston's interpretation destroyed hers since he considers as a substantive hgr which she holds as a divine name. Nor did she see how erroneous it is to mention "WTN" in relation with "wtn;" and she tops her error with the reference to SDDS without even giving the pagination. In fact, she could not have given any page number because SDDS has no entry for wtn as such (p. 165); the only word listed under the wtn entry is the derived noun mhwtn of Ja 627/12 and 628/13 which means lasting running water (cf. JaSDB, p. 439B).

Σ = RÉS 3902, no. 120.

RÉS 3902, no. 120A is republished by Pirenne in CIASA, II, pp. 199-200 (the texts), 201 (one photo) and 235-238 (the three bucrania). The following remarks are limited to the strictest minimum.

1 - The front.

a - The museographic description of the four elements of the front is reduced in Pirenne's description to a skeletonic information. Since she deals at length with the three bucrania, she should at the very least have given their measurements; they are as follows: top: 14.4 x 17.2 cm; bottom left: 11.5 x 18.8 cm; bottom right: 11.5 x 19 cm.

b - The three bucrania are said by Pirenne to be "têtes d'antilopes" (p. 235). Her interpretation manifestly exaggerates the importance of some features, such as the indentation of the horns and their lyre-like form; and she practically does not heed the highly decorative stylization of the whole representation; e.g., there is no way of explaining the central part linking the bases the horns together, and the large width of both the muzzle and theeyes if the head belongs to an antelope. When the head looks like that of an antelope, as in CIH 439, the preceding feature cannot but be the result of the influence of the bucranium. It cannot by itself represent an antelope head. The only valid criterium is the width of the whole head, of the eyes, and especially

of the muzzle. Pirenne, it is true, admits at least in theory that "il peut s'agir d'une stylisation dans la forme des cornes comme dans le mystérieux 'toupet' qui peut être figuré entre celles-ci" (cf. CIASA, I, p. 309). But, her concession is platonic for her interpretation is nonetheless presented as certain.

Pirenne's illogical attitude is also manifested by the following facts. Her basis for excluding the bucranium interpretation of the heads is twofold:"il ne peut s'agir d'un bucrâne [sic!] ., car ce sont des cornes annelées; et les oreilles sont dressées, alors qu'elles sont représentées baissées, pour le bucrâne" [sic!] (cf. CIASA, I, pp. 308-309). This argumentation is valueless and solely for the sake of the cause.

(1) The valuelessness and opportunism of the first element are illustrated by YM 455, of which she herself publishes the photo two pages further (p. 311): the horns of animals are NOT "cannelées." Yet, she declares those heads to be "têtes d'antilopes" (pp. 311 and 312) without even referring to the absence of the first characteristic she herself has so emphatically set forth in the PRECEDING page where she speaks of the "modèles des antilopidés en général" (p. 310). Summarily, Pirenne's descriptions are written exclusively for the sake of the cause.

(2) The same must be said of the second section of her statement. Her "fig. 2" (p. 309) is her facsimile (and an extremely poor one indeed) of the illustration of CIH 695. It should be noted first that her facsimile was retraced on the back of the transparent paper she used to make her initial sketch because the small bucranium is on the LEFT in the original but on the RIGHT in her facsimile. As to the position of the ears, they normally are upward (e.g., Foster 9), but they also are represented either horizontally (e.g., JP 14) or downward (e.g., RÉS 4247). These three examples could very easily have been checked by Pirenne; she did not have to laboriously dig into her files or flip through mastodonic albums to find them because she herself republishes their photos consecutively in the very same CIASA, I, pp. 509, 513 and 515, respectively. Another example of downward ears in Ja 2871.

It should also be added that no proponent of the antelope theory ever included a single reference to a SA word for that animal. Would it not be normal, were the antelope represented so often, that its name would have surfaced here and there in the inscriptions?

The representation of a bucranium, the symbol of the lunar god ʾIlumquh, on an object mentioning the expression qyf/šmshmw have ...] the votive object of their Šams, is no more surprising and does not make more difficulty than the solar disk on an offering to the lunar god.There is no proof that qyf is not a noun.

c - Pirenne repeats her imaginative theory on qyf being "un 'signe' de la divinité, signe extérieur qui permet de reconnaître ou deviner la divinité, interne à la pierre du bétyle (dont le sens est 'maison de la divinité')" (p. 199). Who can actually understand such a nonsense: "un...signe extérieur ... interne à la pierre"? Furthermore, if such is the meaning of qyf, why does she translate the SA word qyf as"qayf"?(p. 199).Who can understand that Pirenne, who always presents herself as inventing unreal things is unable to invent a word or an expression for her concepts? Why did she not make up an expression, such as "un signe extra-interne"?

The main proof of her theory Pirenne gives it in I, p. 309: "cet animal doit être le symbole de ʿAṯtar si l'on se fonde sur deux faits." In fact,however, she mentions three of them.

(1) The antiquities Khalidy-Condé 2 and 3 are memorials erected by two Sab mukarribs at the occasion of a ceremony by which they manifested their sorrow to ʿAṯtar. Yet, it is the symbol of the lunar god which is most prominent: 2 vertical rows of at least 8 ibex heads each contrasting with only 8 antelope heads in the upper horizontal row.

(2) "CIH 439 est dédié à ʿAṯtar et ne cite que lui," states Pirenne. This is not true: the final invocation is bʿṯtr/wbʾlmqh By ʿAṯtar and by ʾIlumquh. Furthermore, the animal head is much narrower and slenderer than in a bucranium.

(3) Finally, Pirenne states that "Taufiq 4 et 5 ... sont des dédicaces à ʿAṯtar dū-Qabḍim et sont précédés d'une tête d'antilope symbolique." Here again, Pirenne distorts the epigraphical evidence.

(a) What precedes TaAM 4 is not an animal head, but a symbol composed of a vertical rectangle with four concave sides which is understood as representing a "door" (cf., e.g., TaAM 2, a dedicace to the three main deities, viz. the stellar god ʿṯtr, the lunar god wdm, and the solar goddess nkrhm) and is the Min symbol of ʿAṯtar according to RÉS 2976 (cf. A. Grohmann, Göttersymbole und Symboltiere auf südarabischen Denkmälern, Vienna, 1914, p. 54). The animal head at the end of the inscription is so stylised that it could belong to any bovine.

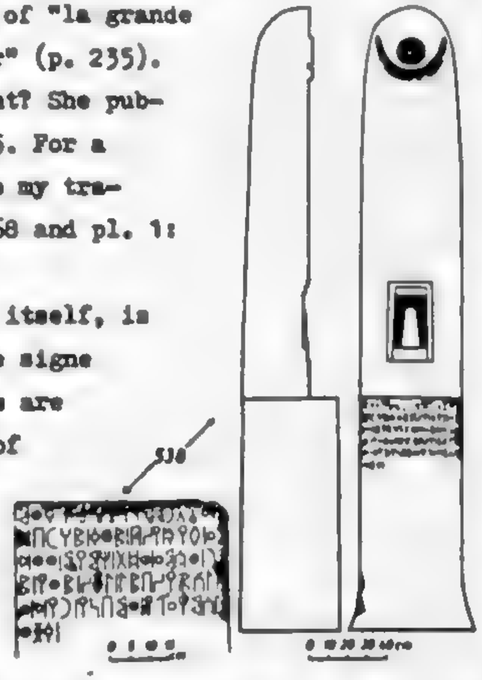
(b) TaAM 5 is a dedicatory inscription to the three main deitites listed in TaAM 2. But, the animal head at the beginning of

the text is so different stylistically from that of TaAM 4 and they both are so far away from the known animal species that they prove how much the carver could stylize the original designs and, therefore, how irrational it is to base the identification of many representations on some details. As a matter of fact, the head of TaAM 5 makes at first sight think of ... a horned fox.

d – Pirenne continues in the same vein and publishes another irrealistic example of her own type of argumentation. She writes this:

[A] «L'inscription du monument RÉS 4635 ... BNY/QYF/ṮTR/WSM'/WḎT/HMYM/WWDM, «il a erigé le *qayf* de 'Aṯtar et de Sami' et de Ḏât-Ḥamim et de Waddum» prouve qu'un seul bétyle pouvait être compris comme le signe de
[B] plusieurs divinités à la fois. Ceci expliquerait que nous ayons ici deux sortes d'animaux symboliques : ceux de 'Aṯtar (la grande antilope) et ceux de la déesse (la petite). » (p. 199)

[C] With reference to "I, p. 309," she speaks of "la grande antilope un symbole du dieu suprême ʿAṯtar" (p. 235).



Who can understand the logic of such an argument? She publishes on p. 236 C. Rathjens' photo of RÉS 4635. For a clearer view of the antiquity, I reproduce here my tracings of Cahiers de Byrsa, 5 (1965), pp. 266–268 and pl. 1: Ja 538.

Ad [A] – The votive object, which is the stela itself, is definitively not, and could not be, "le signe de plusieurs divinités à la fois." Four deities are listed in the text; and the only two features of the stela which are "signs" of deities, are the lunar crescent and the solar disk; thus, nothing visible (cf. her word "signe") is related to ʿAṯtar although ʿAṯtar is the first of the four deities! Pirenne manifestly confuses two different realities, viz. offering to a deity and "signe" of that deity. In fact, the list of deities in the text is nothing but that of many final invocations mentioning ʿṯtr, ʾlmqh and dt/ḫmym adapted by the author to suit his own devotions (as also in Ja 2871, and TaAM 2 and 5), viz. sm' instead of ʾlmqh with the addition of the Min lunar god wdm. Anyone looking at the stela could not find the slightest clue of the fact that it was a votive object erected primarily to ʿAṯtar.

Ad [B] – The statement is a beautiful latius hos. The obvious deduction from RÉS 4635 is that, even when ʿAṯtar is the first recipient of a votive object, the symbol of that very deity is not represented. Yet, it is upon such a basis that Pirenne justifies her finding of the symbol of ʿAṯtar on a votive object which is not even offered to that deity. Furthermore, here again, she distorts the material evidence on two accounts.

(1) The two heads of the lower row do not have the same height. Yet, she represents only one of them (p. 308), and she does not say which one she sketched. No wonder she does not give their measurements.

(2) The scale of the two sketches are wrong as shown by the following schema which gives the proportion between the length and the width of each head.

	original	Pirenne
upper head	1.33	1.24
bottom left	1.64	
bottom right	1.65	1.62

Thus, Pirenne has sensibly reduced the size of the upper head to make the difference between it and the two lower ones even greater than it is.

Furthermore, according to Pirenne's interpretation, the votive offering of RÉS 3902, no. 120A, which is exclusive to Šams, would have two animal heads symbolizing the goddess Šams contrasting with no less than five other heads symbolizing ʿṯtr which is not a part of the offering.

Ad [C] – The text of the reference does not mention at all "le dieu suprême" or any similar or equivalent expression. Pirenne's statement is erroneous: it is the lunar god who is the main deity, as already demonstrated more then fourty years ago in JaP, p. 147.

e – Pirenne's reconstruction of the antiquity in fig. 2 (p. 238) is no more probable than any other, except that the left section has to be duplicated on the right. Her imaginative work is based on subjective considerations, such as this: "la restitution à cinq motifs est toujours plus harmonieuse" (p. 236). In fact, the width depends on the length of text A, and this remains unknown.

f – Pirenne's interpretation of the text is as follows: "D'après notre restitution, il faut supposer 13 signes manquants" and "Il faut probablement restituer: 'X du clan de Y a] érigé le qayf de leur (déesse) Šams" (p. 199). It is immediately apparent that the width of each lower head corresponds to that of four letters. Therefore, the restoration of three more heads would demand at the most 11 letters as there were at least two or three word di-

viders. Finally, her hypothetical restoration is inadmissible because the singular, "a] érigé," does not account for the plural of -hmw, and the normal formula is X/bn [= son of] Y/d [he of (the family/clan)] Z..?..?

2 - The text on the left-hand side.

Pirenne's treatment of text B is simply amazing.

a - She summarizes her theory as follows:

[A] "L'inscription de reemploi
[B] Le côté droit du bloc porte une inscription de 5 lignes qui n'a jamais été signalée
Elle est d'un style graphique très postérieur à la première (cf. infra)." (p. 199). [C]

Ad [A] and [C] - In her argumentation, [A] apparently is the deduction from [C].

The reader has to go to p. 238 to reads this:

" Datation
D'après notre chronologie paléographique, l'inscription originale remonterait au 1er siècle de notre ère (malheureusement l'absence de R laisse quelque incertitude).
La graphie de l'inscription de remploi date du 3e siècle de notre ère."

The style is the same in the two texts: the slight difference to be seen, e.g., in š, is easily explained by the fact that text A is carved in low relief. The style fits nicely in the letter forms used during the reign of šʿrm/ʾwtr at the beginning of the 4th c.A.D. (cf. JaSIMB, pl. I-J and p. 393). Therefore, both her [A] and [C] are erroneous. What is important in Pirenne's argumentation is her juggling of palaeographic types which are superficially studied to fit her preconceived ideas.

Ad [B] - 1 - The first half of her statement, viz. "(le côté droit)" is clearly confirmed by her other affirmation according to which

"Le monument (dont il ne subsiste que la partie droite)" (p. 199. Pirenne's statement is correct is she looks at the back of the antiquity; unfortunately, it is the wrong way to look. And her two statements are contradicted by her later description of the antiquity, viz."cassé à droite" (p. 235).

2 - The second half of her statement to the effect that the inscription "n'a jamais été signalée," which is repeated verbatim on p. 235: "n'a jamais été signalée," is a gross error. It was published separately by J.H. Mordtmann and E. Mittwoch more than fifty years ago in Himjarische Inschriften in den staatlichen Museen zu Berlin, Berlin, 1932, pp. 52-53 and pl. 12, no. 40), and it became RÉS 4155; finally, the two fragmentary texts were reunited in 1971 by JaMAR, II, p. 139, as follows:

"RÉS 3902, no. 120. - Le texte A est reproduit dans RÉS, VI (p. 360).
Le texte B + . + RÉS 4155 est gravé sur la face latérale gauche du panneau supérieur.

1 twbɔl/ɔšwʿ/wb[nyhw/ 7 ou 8 lettres /b]nw/dhlfn/hq
2 nyw/šmshmw/bʿl[t/hrn/qfnhn/wsl]mnhn/lwfyh
3 mw/wl/sʿdhmw/[nʿmtm/wrdw/mrɔh][mw/šʿrm/ɔwt
4 r/mlk/sbɔ/wd[rydn/bn/ʿlhn/nhfn/ml]k/sbɔ/wlwz
5 ɔ/šmsm< sʿdhmw> nʿmt[m/...

1 Tawbɔil ɔAšwaʿ et [ses deux] fi[ls... ,des]cendants de Halfân, ont
2 consacré à leur divinité solaire, maître[sse de Hirrân, ces deux objets votifs et] ces deux [stat]ues pour qu'Elle les pro-
3 tège et pour qu'Elle les rende heureux [avec prospérité et la faveur de le]ur [seigneur,] Šaʿirum ɔAwt-
4 ar, roi de Sabaɔ et de [Raydân, fils de ʿAlhân Nahfân, ro]i de Sabaɔ; et pour que conti-
5 nue Šamsum < de les rendre heureux > avec prospérité [et ...

La restitution de hrn/ qfnhn à la 1.2 est sans doute hypothétique, et l'unique raison de sa mention est qu'elle contient le nombre exact de lettres manquantes; pour šmshmw/bʿlt/hrn, cf. RÉS 4198/4. - wfy (1.2), sʿd (1.3) et wzɔ (1.4-5) sont des infinitifs; pour lwzɔ/šmsm, cf. aussi Geukens 1/26-27. - La restitution de la 1.3 suggérée par J.H. Mordtmann et E. Mittwoch (cf. RÉS, VII, p. 109) contient trop de lettres et un mot doit être supprimé. - šmsm/nʿmt[m: le graveur a oublié sʿdhmw; sʿd est le verbe employé le plus fréquemment dans ce genre d'expression."

The text has an average of 33 letters. Of course, Pirenne pays no attention to JaMAR, as she had already done with Ja 2357 and 2457; cf. JaMAR, XV, pp. 33-39 and 42-43, respectively.

F - NAM 483.

This text, formerly AM 335, was published by G. Ryckmans in Le Muséon, 62 (1949), pp. 62, 64-65, 79 with C.H. Inge's facsimile and photo in pl.2. It was incorporated by J. Pirenne in CIASA, II, pp. 141-144 with a photo in p.143, and an enlargment of the center of ll. 6-10 in color on the cover of part 1. Pirenne's study does not mention the two small fragments belonging respectively to ll.2 and 7-8. It is inexcusable of Pirenne not even to refer in her bibliography to the photo of DoSA, p. 81 (= Südarabien. Antike Reiche am Indischen Ozean, Winterthur/Schweiz, 1970, p. 85) which is more complete than her own photo. The preceding remarks show how subjective is her self-flattering declaration on "la grande familiarité que j'avais acquise avec les objets" (p. III). Finally, she repro-

duces, as if it were the reality, the error of G. Ryckmans' text, "lytb" (l. 8) (cf. below).

Pirenne's skeletonic details of her museographic description are limited to "largeur [sic!, instead of "longueur"] 58 cm, largeur 46 cm" (p. 141). My measurements are as follows: bronze plaque: 58.7 x 46.9 cm; thickness: 0.9 cm; frame: width: 1.9 cm; dentils: upside-down trapezoids: 1.5 x 2.9 x 1 cm; letter height: 3.7 cm; relief: 0.3 cm; interlinear space: 0.9 cm; camel: maximum measurements: 9.5 x 5.7 cm.

Pirenne's reading and translation are as follows (pp. 141-142):

"
Transcription
1) ŠRH'L-W'ZYD'L-W-BNSM/ḌTW/ZYD'L/W'LŠRḤ/-'DMN/Š
2) QNYW/-LSM/WMR'SM/ḌSMWY/'LH/-'MRM/B'MḤRMS/ḤZRBN
3) BTMN-ŠQNYT-ḌḤBN/ḤGN/TKRBS.ŠRḤ'L-W'ZYD'L/BQD
4) MW-ḌN/YWMN/BN/-'RḤM/QBL/LYSM/[W'LY-B]NYSM.BQD
5) MW-YWMN-W'ḌN-ḌSMWY/-'LH/-'MRM/LY[---]ḌTN/-'RḤ/L
6) SM-BḌTN-TKRBS.WŠ[BW-LḌSMWY/W[L-M-T]MNM.RṮD
7) W/ŠRḤ'L/WZYD'L/WBN SMY.ḌSMWY-'DNSMY/W
8) -'DN/BNSMY/WQNYSMY/W ḌSMWY/LY[B.KL.ŠN'S
9) MY/WŠN'/ḌN/QNYW/WBQNYN/WWZ'/ZYD'L/RṮD/MR'S/ḌS
10) MWY/'BLS/DWN/ḌḌYS

Traduction
1) Šarah'il et Zayd'il et leurs fils, tous du clan de Zayd'il et 'Ilšarah, gens de 'DM, ont
2) dédié à leur dieu et seigneur Ḍu-Samāwī, dieu d'oracle, dans [son sanctuaire]
Ẓarbān,
3) à Tomna', une dédicace en «or», comme Šarah [']il et Zayḳd'il s'y sont engagés
envers lui au-
4) paravant, à propos d'une affaire (où) il leur avait fixé compensation, à eux et à
leurs fils, au-
5) paravant, et qu'il avait été entendu (avec) Ḍū-Samāwī, dieu d'oracle, qu'il
[arrangerait"] cette affaire pour
6) eux en contre-partie de cet engagement envers lui. Et ils témoignent leur
reconnaissance à Ḍū-Samāwī et 'Am.
6-7) Šarah'il et Zayd'il et leurs fils ont confié à Ḍū-Samāwī leurs facultes et
8) les facultés de leurs fils, et ce qu'ils possèdent. Et que Dū-Samāwī punisse tout
ennemi
9) de leurs deux personnes, ou ennemi de ce qu'il possèdent ou possèderont. Et en
outre. Zayd'il a confié à Ḍū-Sa-
10) māwī son chameau 'Adawān, de ce qui est à lui."

L. 1: dtw: "dty" of G. Ryckmans' transcription in SA letters (p. 62) is printing error instead of "dtw" (p. 64). Pirenne's translation of the word as "tous" also is erroneous; the idea of "tout/tous" is always expressed by kl; "ceux" would have been correct. — ʾʾdmn: G. Ryckmans translates it as "sujets," and specifies that "le mot ʾʾdmn n'a pas de régime: il désigne simplement la classe à laquelle appartiennent les dédicants" (p. 64). Pirenne translates it as "gens de ʾDM" (p. 141). Here again, Pirenne does not seem to understand the words she uses since the first phrase of her commentary (p. 142) states:

«'DM en tant que pluriel de 'DM, sujets, vassaux ou dévots à une divinité.» For ʾʾdm, cf. already above, p. 15. Pirenne's statement on ʾdm is erroneous: it is a well-known fact that ʾdm is also said of human beings; cf., e.g. Ja 689/1: ʾdm/bn/ḥ̱lḥ̱n; 696/1-2 and 697/2: ʾdm/kbr/hll; 704/2: ʾdm/bn/hmdn; etc. Whether or not invited by Pirenne to help her in her understanding of that word, which is, of course, a major difficulty, A.F.L. Beeston intervenes again as follows (p. 142):

[A] «It is almost impossible that 'DM should be rendered as 'les sujets'. 'DM is itself a collective, and though a 'plural of a plural' is a phenomenon that does occur in Arabic, its occurence is restricted, and 'DM in the sense 'subjects, clients, vassals' always has a genitive following, which names the person(s) of whom they were subjects: without this specification, the term would be meaningless. I would go so far as to say that it is certain that 'DM, here, is the plural of a tribal designation 'DMY-N, based on some such name as 'DMN attested in RÉS 4351/1 as the name of a valley (cf. the modern toponym: Wadi 'Idim?)» [B1] [B2] [B3] [C]

Ad [A] – This statement is the formulation of his thesis. His proof is given in three successive affirmations.

1 = [B1] – After reaffirming the well-known fact that ʾʾDM is itself a collective," his initial thrust is taken from Ar. What is the value of such a statement? Were the pan-Ar Beeston consistent with himself, he should of course recognize as much possibility in SA as he does in Ar and especially since such a feature does exist in SA; cf. above, p. 7, the note on gny. The only value of his previous statement is to indicate that, on the one hand, ʾʾdm as the secondary plural of the collective ʾdm is paralleled by the examples of the same feature in Ar and, on the other hand, that in SA as well as in Ar the cases of such a plural are very limited. In fact, thus, Beeston's statement is contrary to his thesis.

2 = [B2] – Beeston's statement, "always have a genitive following," has in his mind the value of an absolute exclusivity, viz. "must have a genitive following," as proven by his following affirmation. But, that is precisely his thesis which is unacceptable because his reason is invalid.

3 = [B3] – Beeston attempts to justify the exclusivity of his "always" by stating that "without this specification [= the genitive following ʾdm], the term would be meaningless." Here again, Beeston takes his ideas for the reality. Even a child can understand that being "servants" is as much a professional status as being ʾqwl rulers or ʾsbʾ soldiers or ʾmlk kings. Therefore, for Beeston, ʾʾdm servants would be as "meaningless" as ʾsbʾ soldiers or ʾqwl rulers or ʾlmk kings. All these words refer to professional classes or ways of life. Of course, nobody

can actually be a qwl ruler without a clan or tribe to rule, or be sb³ soldier without belonging to an army, or be mlk king without a kingdom; and therefore, no persons can be ³³dm in reality without serving a master. That is the reason why the dependency is normally specified by a genitive complement listed after the word itself. But that dependency can also be expressed without that genitive. The present text is a good example of such a case. Šaraḥ³il and his wife Zayd³il and their sons (bn of ll. 1, 7 and 8 is a plural because of bny of l. 4) were at the time of the bronze offering a part of (dtw) the family of another couple, Zayd³il and his wife ³Ilšaraḥ, AS servants. The difference between the two following expressions: šarḥ³l/wzyd³l/wbnsm/dtw/zyd³l/w³lšrḥ/³³dmn (the text) and " " " /³dm/zyd³l/w³lšrḥ is that the authors of the first expression are professional servants temporarily at the service of the couple who hired them, but the authors of the second expression are permanent servants of the couple for whom they are now working. An even clearer case is RÉS 4771, a royal edict given to a clan on business transactions ʿmn/kl/³l³(t)m/w(³)mlkm/w³qwlm/w³šʿbm. NO specification at all is given to any of the four words of the series, and there could not be any because such details would have necessarily limited considerably the purpose and the goal of that royal edict.

Ad [C] – The preceding remarks prove how irrealistic and subjective Beeston's conclusion really is. The main phrase is nothing but a rehash of his thesis. Furthermore, Beeston apparently does not see how abnormal it is to attempt to explain an ethnical affiliation with the name of a ... valley! Moreover, ³dmn is in an identification without any proof, without telling him that the origin of RÉS 4357 remains unknown, and all the more so that the reader does not even know whether Beeston's "Wadi ³Idim" is Wâdî ³Adîm (south-southwest of Taʿizz) or Wâdî ʿAdim (with ʿayin; south of Tarîm in Wâdî Ḥaḍramawt).

L. 2: ³lh/³mrm (also l. 5) is translated by Pirenne as "dieu d'oracle," a rendering which she copied from G. Ryckmans (p. 64) who explains his translation as follows: "il serait étonnant qu'après le transfert de son culte en pays qatabanite, ce dieu reste qualifié d'un nom sabéen "(p. 64). Such a remark is invalid: there is no reason why the god could not be mentioned with his original title. Furthermore, the word ³mr means command, order (cf. above, p. 15) regardless of the way the command/order was transmitted to the faithful. Finally, it is also one of Pirenne's fads to translate the proper names in the identity cards of deities.

— bn[ḫrms]: already restored by G. Ryckmans (p. 62) and copied by Pirenne as "B[MḪRMS]" (p. 141), and not a single word of commentary to justify her elimination of n. Indeed, the initial n of the word is NOT to be seen on the photos published by G. Ryckmans, Doe and Pirenne; and authors who, like Beeston and Robin, swear only by what they see on photos and exceptionally read a text from the original should reject that initial n of C.H. Inge's facsimile (p. 79 of G. Ryckmans' paper) as a dittography of the preceding b... and Pirenne did not see it... at all. Yet, Inge's copy is correct, but the published photos did not register the letter because it is on a tiny fragment which I have seen and registered in my copy. The preceding remarks also apply to both s of dsmwy (l. 7) and l of lyatb (l. 8; cf. below)...because those two letters are not to be seen on the published photos. Yet, here again, Pirenne says nothing about them ... as if everything was normal. These two letters were on one elongated fragment which I also saw and registered. Pirenne's complete silence on those three letters proves the inexcusable carelessness of her work: instead of being blinded by her own classification and sigla, she should have heeded the antiquity itself and Inge's facsimile and, if necessary, have initiated a search for the missing fragments and have told the reader what the situation was. — krbn: G. Ryckmans reads "ẓrbn" (p. 62) and comments as follows: "ẓrbn; le ẓ est attesté par la copie; la photo permet également de lire un ẓ, de préférence à un k ou à un s" (p. 64). These statements are untrue: Inge's facsimile (p. 79) reproduces what the photo shows, viz. a bottomless rectangle + a tiny aslant bar extending the left vertical side of the rectangle. Note that the tiny piece of the plaque immediately above the rectangle which is missing in both G. Ryckmans' and Pirenne's photos is present in Doe's photo where the diacritical circle of ẓ is not to be seen. The letter ẓ is excluded, but there was no justification for excluding k and preferring ẓ, as Pirenne does; cf. her "ẒRBN" in p. 142, and all the more so since she herself states that "le ẓ n'est pas sûr; ce pourrait être K" (p. 142). The simple fact is that the letter is k and cannot be read ẓ. krbn is known as the name of a person in Min and Sab.

L. 3: sqny/dhbn is erroneously translated by Pirenne as "une dédicace en 'or'," which is her adaptation of G. Ryckmans' "une offrande d'or" (p. 64). Note Pirenne's expression "en 'or'" (here), but "'d'or'" (pp. 148, 150 and 154). The only apparent reason for adopting a special expression here is that she did not wish to show her dependency from G. Ryckmans. That sqnyt does not have the article is due to the presence of the genitive dhbn. However, there cannot be any doubt that sqnyt means this offering because it is most obvious that sqnyt refers

to the bronze plaque itself. Furthermore, SDDS (p. 38) still maintains "gold" as the first meaning of dhb but, surprisingly enough, does not include J. Ryckmans' rendering of the word as "doré" (cf. Le Muséon, 66 [1953], p. 359) = "golden" in Beeston's translation of Doe 2/3 (cf. Raydān, 4 [1981] 1982, p. 13). That dhb cannot mean "gold" is staring at the face of Pirenne, Ryckmans, Beeston, SDDS, etc is that - and they all know it! - there has never been any indication or trace of gold applique or covering on any bronze object ever found; and it could not possibly have been; it would indeed be stupid to think of a thin gold sheet covering an inscription, as here. Since the plaque is manifestly of bronze, their translation of dhb as "gold" or "doré" or "golden" cannot but be the result of plain dishonesty. The present case is yet another justification of the position I have always held, viz. dhb is bronze; cf. in particular, my refutation of G. Ryckmans's long vindication of his dhb = "or" (cf. Le Muséon, 70 [1957], p. 106) in Quatre inscriptions sud-arabes, Washington, 1957, pp. 3-4.

L. 3: tkrbe is translated by Pirenne as "s'y sont engagés envers lui." There is no correspondent of the personal pronoun "y" in the text; and tkrb means to attribute to. Note also that Pirenne completely drops my initiative of capitalizing the initial of all pronoun referring to a deity, although she had adopted it sporadically in CIASA, I (cf. my review in JaCIASA, p. 265)(cf. also above, p. 14).

Ll. 3-4: bqdmw/dn/ywmn = bqdmw/ywmn of ll. 4-5, is translated by Pirenne as "auparavant." Such a rendering is inaccurate because, as such, this adverb, "previously, in the past," means only the anteriority of an action over another. But, such a vague anteriority is already indicated by bʿn which introduces the cause of the offering and, obviously enough, a cause always precedes its effect, but not necessarily in the immediate past. The specific meaning of the present expression is that the cause motivating the offering happened "prior to this day," thus, in the immediate past, not sometime in the past. Furthermore, Pirenne's literal translation of the expression bqdmw/dn/ywmn as "avant ces jours" (p. 144) also is erroneous because both dn and ywm are singular, not plural at all.

Ll. 4-6: Pirenne's translation, which in fact implies the repetition of l. 4 in l. 6, is another piece of wild work and is unacceptable because of the following reasons:

1) the plural ʾʾrḥ is translated by a singular, "affaire." It looks as if Pirenne does not seem to know the difference between the numbers!

2) The subject of the verb qbl is ʾʾrḥm, not the god at all.

3) The translation of ʾdn/dsmwy as "il avait été entendu (avec) Dū-Samāwī" with an impersonal subject as if someone (but who?) had bargained on the matter with the deity. Such a grammatical construction is foreign to SA. ʾdn is a noun followed by the genitive dsmwy.

4) tkrbe is a verbal form, not a noun.

5) stbw [t]ʾmnm: Pirenne's translation as "ils témoignent [in the present tense!] leur reconnaissance" is a dramatic presentation of erroneous translations.

L. 4: qbl ʿly, cf. Ar "qabala as syn. of ʾaqbala" and the 4th form "ʾaqbala ʿalayhi he clave to it" (cf. LaAEL, p. 2983A-B). In her quotation of LaAEL's texts (p. 2983A), Pirenne distorts the verb "requited" to "required"! Furthermore, Pirenne's understanding of the subject of the verb qbl as being the god himself is unjustifiable: the name of the deity would have followed qbl.

L. 5: ʾdn/dsmwy: as ʾʾrḥm, ʾdn depends on the preposition bn of l. 4. - Pirenne reads "LY[.....]YTN" with six dots, thus two letters more than in G. Ryckmans' text: "ly[../wb]dtn" (p. 62), with a most confusing and inaccurate translation: "eu égard à ce que Du-Samawi leur avait ordonné," in which ʾrḥ is considered as a verb. The lacuna has 4 (at the most 5) letters because the lines have 35 or 36 letters in ll. 1, 4 and 2, 3, respectively. Note, however, that ll. 7-8 with the camel are more crowded with 31 letters each + the space occupied by the camel which equals the width of 8 letters.

L. 6: stbw [t]ʾmnm, cf. Sab htbw tʾmnm of Ja 716/9. Pirenne refers to SDDS, p. 151 'témoigner sa reconnaissance'." The idea of "témoigner" is alien to the root twb (cf. also below, the commentary on Ja 3198/3 in G), and that of "gratitude" also is alien to the root ʾmn (cf. JaSIMB, p. 20B); cf. also DAPA, I: "Notion de base: sûreté, sécurité, confiance" (p. 229A), and in the pages 229-238 devoted to the root ʾmn there is never a word even remotely referring to "gratitude;" cf. also D. Cohen, Dictionnaire des racines sémitiques, Paris, I (1970), p. 23. Therefore, JaSIMB's translation of tʾmn as "testimony of confidence" (p. 449B) is justified and maintained here also. — Pirenne's restoration of "l'ʿm" after ldsmwy/ which she copied from G. Ryckmans' p. 62, is unjustifiable: the god ʿAmm is never mentioned, even once, in the text which consistently contains only the divine name dsmwy in ll. 2, 5, 6, 7 and 9-10. Furthermore, that restoration is contradicted by the number of letters in the lines the length of which is ascertained (cf. abo

ve, the commentary on l. 5). The lacuna before t'mn is of 5 (at the most 6) letters. There is no way of knowing what could be restored in the lacuna.

The pericope of ll. 3-6 means this: the bronze offering was made possible with what the three authors had set apart from their activities of the immediate past and this setting was also due to the deity's favorable attitude toward that attribution to be utilized in the making of the bronze offering.

L. 8: the error of G. Ryckmans' text, "lytb" (p. 62), which is correctly read in the commentary (p. 65): lystb, is repeated by Pirenne in both her translitteration (p. 141) and her commentary (p. 144) although the photos published by both G. Ryckmans and herself as well as C.H. Inge's facsimile have lystb. Pirenne translates the verbal expression as "que . punisse"(p.142),but G. Ryckmans renders it as "que . livre" (p. 64) Both translations are made up from the context at the expense of etymology because G. Ryckmans' "livrer" means "to deliver, hand over," and Pirenne's "punir" "to punish." The basic meaning of Ar tāba (o) is "to return (to the starting point)" (cf. DAFA, II, p. 1252B). The intensive verbal form must have the meaning of the 4th form, viz. "to make (someone) to return (to the starting point), viz. the verb may be translated as to remove away, keep away, send away.

Ll. 8-9: the personal pronoun -smy affixed to both qny and ʾsn³ must be noted.In the preceding lines, the couple and their sons have constantly been united in all facts related. This situation, however, seems to have been more honorific than real for the sons: they do not have any slave of their own, -smy affixed to qny slaves (l.8) refers to the parents only; and they have no personal enemy, -smy of ʾsn³smy (ll. 8-9) again refers to their parents only. This can only mean that they were still infants.

L. 9: dn before qnyw is translated by G. Ryckmans as "de ce que" (p. 64),with no remark in the commentary of p. 65. The preceding interpretation is copied verbatim by Pirenne and, here again, there is no remark in her commentary(p.144). Yet, dn is masculine; thus, this demonstrative pronoun refers to the collective noun qny slaves of l. 8. Such an interpretation is also demanded by the context because inanimate things have no ʾsn³ enemy. Furthermore, the pericope ʾsn³/dn/qnyw confirms my interpretation of qny as slaves (cf. above, p. 7), and not at all "possessions, cattle, livestock," as in SDDS (p. 106) or "avoir" and the plural ³qny as "objets," as J. Ryckmans in Sayhadica, pp. 169 and 170, respectively. These translations are excluded by SA contexts because "avoir" also includes money (which is never mentioned in such contexts), and the plural "objets" is also excluded because "avoir" includes also all livestock. — wz³/zyd³l/rtd is translated by G. Ryckmans as "Et Zayd³il a en outre voué à." This rendering distorts the actual meaning of the text by implying that it is the second phase of the action taken by Zayd³il, and that it started right here. In fact, the verb wz³ does not have any of SDDS's meanings, which were elaborated on the basis of G. Ryckmans' interpretation, viz. "do s.t. again, furthermore, in addition, in future" (p. 167), to which Pirenne refers (p. 144). The verb means here the continuation of an act already existent, therefore, not "again" (as if it had ceased to be at one point), not "furthermore, in addition, in future" (cf. above, my answer to G. Ryckmans).

L. 10: "³bls/ʿdwn/ddys" is translated by G. Ryckmans as "son chameau ʿAdawān qui est des siens(?)" (p. 64). This rendering involves a manifest tautology, viz. "son" et "qui est des siens(?)" beside the rendering of the singular pronoun -s as the plural "siens." Pirenne's translation as "son chameau ʿAdawān, de ce qui est à lui" is inspired by G. Ryckmans' and repeats the latter's tautology,but restores the meaning of the singular -s. But, she subreptitiously introduces a preposition, "de," which does not exist. On the question of the so-called second "d," G. Ryckmans' commentary states that "C.H. Inge, après vérification sur sa demande, confirme la lecture rendue par le facsimilé" (p. 65). The facsimile reproduced on p. 79 does not justify G. Ryckmans' reading of the antepenult letter as "d" because Inge's facsimile shows two different letters.The letters are:

first, d with both extremities free of any crossbar joining them together and two short aslant bars in the center; another longer, adventitious aslant line crosses the upper half of the right vertical staff;

second ḏ with both extremities closed by a V-shaped broken line making one sharp angle each, and two aslant bars inside the rectangle; it is an embellishment of the type of RÉS 4708A/2 (cf.below).

Thus, Inge's facsimile is accurate enough to show that the two letters are not alike and, therefore, cannot be given the same value. The whole of l.10 is clear in the color photo printed on the cover of part 1 of CIASA.

The letter ḏ in the text, , is constantly normal, and in each of the eleven ḏ's the two extremities of the letter do not show a crossbar, as expected.

The end of the text reads as follows: ³bls/ʿdwn/ḏḏys. For ḏys, cf. Ar ḏayṣa (i) "decrevit et arescere coepit herba" (cf. FrLAL, III, p. 333). Since the name of the camel's former owner would not normally have been mentioned as such, ḏys may

refer to the breed of the animal which may originally have been the name of either a place or the individual who initiated it.

The text may be read as follows:

1 šrḥ'l/wzyd'l/wbnsm/ḏtw/zyd'l/w'lšrḥ/''dmn/s
2 qnyw/'lsm/wmr'sm/ḏsmwy/'lh/'mrm/bm[ḥrms/]krbn
3 btmn'/sqnyt/ḏhbn/ḥgn/tkrbs/šrḥ['l/wzy]d'l/bqd
4 mw/ḏn/ywmn/bn/''rḫm/qbl/'lysm/[w'ly/b]nysm/bqd
5 mw/ywmn/w'ḏn/ḏsmwy/'lh/'mrm/l[y][..../]ḏtn/'rḫ/l
6 sm/bḏtn/tkrbs/wstbw/lḏsmwy/w[.....]'t''mnm/rtd
7 w/šrḥ'l/wzyd'l/wbn' [illegible] smy/ḏsmwy/''ḏnsmy/w
8 ''ḏn/bnsmy/wqnysmy/w [illegible] ḏsmwy/lystb/kl/šn's
9 my/wšn'/ḏn/qnyw/wbqnyn/wwz'/zyd'l/rtd/mr's/ḏs
10 mwy/'bls/'dwn/ḏḏys

1 Šaraḥ'il and [his wife] Zayd'il and their sons, those of Zayd'il and [his wife] 'Ilšaraḥ, the servants, ha-
2 ve dedicated to their god and their lord Ḏû-Samâwî, god of 'Amrum, in [His] te[mple] Karbân
3 in Timna', this offering of bronze according as had attributed to Him Šaraḥ-['il and Zay]d'il prior
4 to this day from the enterprises [which] had claved to them [and to] their [s]ons prior
5 to this day and [from] the understanding of Ḏû-Samâwî, god of 'Amrum, that .[...] this enterprise [= the offering] for
6 them in this attribution, and they presented to Ḏû-Samâwî ... a testimony of confidence. Have entrust-
7 ed Šaraḥ'il and Zayd'il and the sons of them both to the care of Ḏû-Samâwî the understanding of them both and
8 the understanding of the sons of them both and of the slaves of them both. As to Ḏû-Samâwî, that He may keep off all enemy of them
9 both and (all) enemy of that [= every slave] which they have possessed and shall possess. And Zayd'il has continued to entrust to the care of his lord Ḏû-Sa-
10 mâwî his camel 'Adwân, it of Ḍays.

G - NAM 224 = Ja 3198.

The Qat text AM 216 published as NAM 224, is fragmentary on all sides; its surface is polished as if by usage and covered with an inscription more or less worn out, very often to the extent of being illegible, a challenge even for any epigraphist facing the stone. It is temerity for anyone to attempt any decipherment from a mere photograph. A.F.L. Beeston published his attempt in CIASA, II, pp. 225-226 and photo on p. 226. The present text is another proof of the error of C. Robin's statement according to which "une publication d'inscription sans photographie est inutile" (cf. JaMAR, XIV, pp. 271, 286, etc.; cf. also below, IIIA8).

1 - Museographic information.

The following measurements must replace those given by J. Pirenne (p.225). Maximum height: 29.6 cm; width at the horizontal axis of l. 5: 24.5 cm; letter height: 3.1 and 3.3 cm.

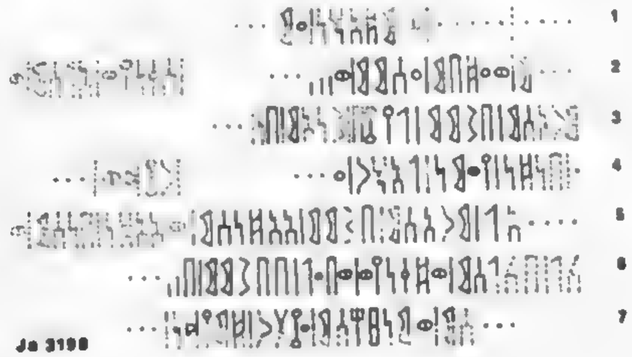
2 - Beeston's reading.

* Transcription	Translation
1) [W]MḎ[R] ...	1) and? 'MḎ'R? ...
2) M.W'ḎḎM.WSLMN ...	2) and 'ḎḎM and SLMN
3) SM.BTM.L[T]TSM...	3) [because He granted] them a daughter for [their] wife.
4) ḎN.YWMN.L'HR.W...	4) [from] this day henceforth, and ...
5) 'L.MR'S[M].B ...	5) their lord. ["the lord of BŠMM"]
6) LSM.ḎQNYW.WB'L.ḎBŠMM ...	6) for them that which they possess, and the lord of BŠMM
7) M.WMNḎHSM.'TTR.ḎM ...	7) and their south 'TTR of M... *

Beeston contradicts himself by considering the beginning of each line as ascertained: the seven lines of his text are well aligned on the left in spite of his statement according to which the stone "is broken on all sides" (p. 225). - He characterizes the text as "?Dedication in thanks for a daughter?" (p. 225; cf. also p. VIII of CIASA). Such a title is based on the editor's erroneous reading and restoration of l.3. - Beeston's reading of the inscription must be corrected in thirty places and must, thus, be discarded as a whole. Beeston's workmanship is also illustrated by his reading of NAM 2694 (cf. CIASA, pp. 205-207, and photo in p. 207), viz. of "SQNYTM" in l. 2 (p. 205), and his commentary (p. 206) claims that "Jamme's text erroneously omits the m from sqnytm." No less than four lines of his commentary are devoted to explaining his expression "sqnytm/ḏhbn" according to Ar grammar, and the 3 1/2 lines of footnote 1 labor the same question. Unfortunately, the "m" does NOT exist in the photo. - As the proof of the "importance of the document" (p. 226), Beeston states that "this is the first time that we have encountered a specific name given to a mnḏḥ." The author eliminates the epigraphical evidence already gathered in JaP, pp. 126-127; cf. also JaMAR, XIV, pp. 101-106 (Beeston on Qat H 2 c) and 271-274 (on C. Robin's and A. van den Branden's theories).

3 - My decipherment of the original stone to which are added a few

restorations is given below. However, the basic handicap being the impossibility of knowing the exact place of the fragment in the original stone and, therefore, the impossibility of knowing the number of letters missing at both the beginning and the end of the lines, the length of the lines in my facsimile remains conjectural although my restoration of ll. 2-3 and 5-6 is based on published texts.



Ja 3198

The text may be read as follows:

1 [..../...../w]⌜md³h⌝[n]ᵢ/ⱼᶜᵢmⱼ[...
2 [...]⌜m/⌝wᶜ⌜d⌝bm/ᶜsmm/..[... /sqnyw/³lsm/w]
3 [mr³]ᵢsm/bšmm/ly⌜tb⌝/[šn³]mᵢ/ⱼb[n/...
4 [./bn]ᵢdn/ywmn/l³⌜hr/ᶜ.[... /rtdw/...
5 [....]ᵢ³l/mr³s⌜m⌝/bšmm/³³dnsm⌜/⌝[w³³dn/bnsm/w]
6 [kl/bk]ᵢlⱼsm/wdqnyw/wbᶜl/bbšmm/b.[...
7 ...s]ᵢm/ⱼwmnⱼḏḥᵢsm/ⱼᶜtᵢtr/dmyd⌜n⌝ᵢ/ⱼ[...

1 ... and] Muda³³ih[ân] ᶜAm[...
2 [...]m and ᶜAdbum ᶜAssum and ..[... have dedicated to their god and]
3 their [lord] Bašamum so that He may keep away an(y) [enemy] fr[om ...
4 [. from] this day to [the] end. [... Have entrusted ...
5 [...](³il) to the care of their lord Bašamum their understanding [and the understanding of their sons and of]
6 [all] their [affi]liates and what they have possessed and owned by Bašamum in .[...
7 ... the]ir ... and their irrigation-deity ᶜAttar, Him of Maydân(?) [...

L. 1: md³h[n], as in Ja 1191/2, a rock inscription from the country of Mukérâs; cf. Ar dayyaha "vilem reddidit, submisit aliquem" (cf. FrLAL, II, p. 101A).

Ll. 2-3: ³lsm/wmr³sm, as in RÉS 4336/1, another dedicatory inscription to the god Bašamum; l. 2 of this text also justifies the restoration of mr³sm alone before bšmm of l. 5, the space being too wide for three letters only; it demands four letters; thus, mr³s as restored by M. Höfner, in WZKM, 42 (1935), p. 44, followed by RÉS, VII, p. 197.

L. 3: bšmm: Bašamum is a god whose attributes remain unknown (cf. JaP, p. 140). - lytb/[šn³]m, cf. NAM 483/8-9: lystb/kl/šn³smy (cf. above, pp. 36-37).

L. 4: bn]dn/ywmn/l³hr, cf. bndn/wrhn/l³hr of Ja 2360/3.

L. 5: ³³dn is in the plural as referring to the ³dn of each person. Beeston errs on both nfs and ³dn in Ghul 4/3 (cf. JRAS, 1985, pp. 149-150) where he translates the words as "soul" and "material faculties," respectively. nfs simply means person; the word "soul," which is the first translation given to the noun by SDDS (p. 93), must be avoided because of its basic meaning referring to an immaterial entity which is completely foreign to the strictly materialistic aspirations expressed in all texts. Furthermore, Beeston's expression "material faculties" is unintelligible. It is also interesting to note that Beeston translates ³dn in Doe 2/4 (cf. Raydān, 4 [1981] 1982, p. 13) as "senses," and the reader does not know which meaning Beeston gives to the word. Finally, the last two interpretations given by Beeston are not listed under the word in SDDS, p. 2.

Ll. 5-6: for ³³dn/bnsm, cf. NAM 483/5, where ³³dn/bnsmy also follows ³³dnsmy; for wkl/bklsm before dqnyw, cf. RÉS 4336/5.

L. 6: dqnyw/wbᶜl, cf. kl/dm/qny/wbᶜl in Doe 2/4-5.

L. 7: the appellative of ᶜAttar, the stellar god, is tentatively read as mydn; cf. mwdm in Wâdî Mâsil 1/6.

III – Review of Sayhadica.

Sayhadica is a booklet of XXXIV – 180 – 1f pages offered to A.F.L. Beeston "par ses collègues" (p. V). Composed of 16 papers, including a short "Autobiography" (pp. XV-XX), it is edited by C. Robin and M. Bâfaqîh or M.A. Bâfaqîh and C. Robin depending on which front page the reader looks at, either the French (p. V) or the Ar (page 0). C. Robin kept the lion's share with 69 out of the 200 pages of text and illustrations.

A – C. Robin.

It is toward the end of JaMAR, X, that my evaluation of Robin's work is summarized in uncertain terms (pp. 65–66): "SA science, therefore, has to further brace itself against that second generation's onslaught which is only in its beginning" (p. 66). As it was fully expected, the flood tide is still coming, as exemplified by JaMAR, XIV, pp. 266–302, which may serve as my review of Robin's thesis ,RoPH, which was reshaffled in his 1982 publication, RoHT. The following examples illustrate that the flood tide claims new victims as time passes. The following examples are divided into ten sections. The insertion of two examples from another publication is aimed at reminding the reader – if at all necessary – that the present type of aberrant behavior is well in the line of those denounced in the past few years; viz. those examples bridge the present study with that of JaMAR, XIV, pp. 269–303.

1 – Sayhadic, Sayhadica, Sahidic.

a – In his BeSC (1982), Beeston states that "I suggest that an appropriate designation for [the SA texts] might be Sayhadic ... more convenient than the somewhat cumbrous 'Epigraphic South Arabian' current hitherto" (p. 1). The "cumbrous 'Epigraphic South Arabian'" was his own invention, which he normally shortened to "ESA" (cf. below). If he now thinks that his own invention is "cumbrous," why does he not accept what other authors do and simply use South-Arabian or "sud-arabe"? How could such appellations be "cumbrous"? But, what is actually not only "cumbrous" but also despicable is his distorting and pan-Ar argumentations. Then, he states that the SA languages "have come to be designated by scholars as Minaic (or Minaean), Sabaic, Qatabanic

... and Hadramitic" (also p. 1). This generalization is a plain lie: Beeston is the only one using the four adjectives with the -ic ending; and he is not even consistent with himself, since he uses, e.g.,

"Sabaic" in Raydān, 1 (1978), pp. 18 and 24;
"Sabaean" in l.c., 3 (1980), p. 3, and back to
"Sabaic" in Arabica, 28 (1981), p. 181.

In other words, Beeston goes from one to the other and back to the first as he has done with the sibilants, as denounced in JaMAR, XIV, p. 70. It is not worthy to collate the use of those five adjectives in Beeston's publications because, starting in 1986, Beeston's unstable mind begins another cycle of wavering expressions.

(1) In CIASA, II (1986), p. 277, Beeston's unsteady mind aoandons his "Sabaic," to speak of "Sahidic language." His text is reproduced above, p. 22. There is not a single word to inform his reader that "Sahidic" is a novelty by which he turns his back to, and repudiates, his own "baby" "Sabaic."

(2) In a worthless and useless paper entitled "Apologia for 'Sayhadic'" (cf. PSAS, 17 [1987], pp. 13–14), Beeston, after repeating Erastothenes's testimony, which is irrelevant to the matter and which he had already referred to in his 1982 BeSC, formulates his snobbish thinking as follows: "the desirability of distinguishing between linguistic and non-linguistic data suggests the preferability of referring to linguistic data as Minaic, Sabaic, Qatabanic and Hadramitic, leaving Minaean etc. for non-linguistic contexts."

(a) That "desirability ..." is irrealistic and unreal: the only reality it may have is in Beeston's mind which needed so many years to find out that his own formula was "cumbrous." As the five words involved basically are adjectives, they normally are used with nouns and these nouns always specify clearly the object itself. Even when the adjective is taken as a noun, the context never leaves any doubt about the subject; e.g., Sab/Sabaean used in the commentary of a word can only refer to the language; but when found in an historical context, the word necessarily refers to the culture or history. Therefore, it is silly to even think of making explicit that which is already explicit.

(b) He distorts the value of the English adjectival ending "-ic" which, as well as the French "-ique" (cf. already JaMAR, XIV, pp. 63–64 on M. Rodinson's "sudarabique"), means only "relat-

ed to, relating to, belonging to, associated with" and "relatif à, propre à," respectively.

(c) There is no justification - and Beeston does not give any - for his connecting "-ic" with language and "-an" with the culture.

(d) Nobody in the past has been pedantic and/or deranged enough to suggest the use of the following expressions: "langue francique/culture française," "lingua italica/cultura italiana," "americ language/american culture," etc.

(e) Beeston's suggested "Saynadic" is also inaccurate for he limits the value of his forged adjective to a section of the south and southwest of the Ar sayhad ... as if, e.g., the Tham inscriptions south and southeast of the sayhad did not exist.

(f) Beeston also writes that "the terms Epigraphic South Arabian (ESA) or Old South Arabian ... (OSA ...) have now gained general currency" (p. 13). Beeston knows very well that his statement is a lie: those are his own expressions.

(g) There is not the slightest reference to his own "Sahidic" printed in 1986, the very same year he delivered his paper of PSAS published in 1987.

(3) The very same year 1987, Robin states that the booklet offered to Beeston "ne pouvait s'intituler que Sayhadica" (cf. Sayhadica, p. XII). This statement which reflects what may be called an obvious self-evidence is contradicted by the question mark in the title of Robin's contribution, "SAYHADICA?" (p. XI; cf. also p. VII), and nothing in pp. XI-XIII explains or justifies the question mark which is not even mentioned and disappears in the booklet. Therefore, the reader still does not know what troubled Robin. Furthermore, the statement was already proven wrong because of 1986 "Sahidic" of Beeston himself, an appellation which Robin obviously does not even mention. Finally, the apparent evidence seen by Robin did not seem quite so apparent and so compelling to the contributors of the booklet among whom two belong to the "bande des quatre" (p. XII); cf. "ESA" of Y. Abdallah (p. 6), "ancient South Arabian" of A. Avanzini (p. 14), "epigraphic South Arabian" of A.G. Lundin (p. 49), "altsüdarabisch" and "südarabisch" of W.W. Müller (pp. 64 and 65), "sud-arabe" of J.Ryckmans (p. 165) and even ʿarabîyat an-nuqûš of M.A. Bâfaqîh, one of the two coeditors (p.V). Note that this Ar expression is translated by the coeditors as "sudarabique épigraphique" and Ar al-ʿarabîyat al-fuṣḥá of M.A. Bâfaqîh as "l'arabe"(p.V). Finally, the expression "bande à quatre" reported by Robin also is noteworthy. Since Oxford is the place where the expression was coined, why did Robin not give the original English wording instead of using a French translation? Could it be the result of a personal dislike of things English? At any rate, it has to be a word similar to "club" of my expression "laudemus invicem club members."

2 - Beeston's "bibliographie."

After his above-mentioned paper on "SAYHADICA?," Robin devotes 14 pages (pp. XXI-XXXIV) to "A.F.L. BEESTON Bibliographie (1937-1986)." The reader remembers that he had already done something similar in CIASA, viz. "Bibliographie générale systématique" (Louvain, 1977). For this bibliography, cf. my review in Orientalia, 48 (1979), pp. 278-282. The same quality of workmanship is also found here. Let us concentrate our attention on "Recensions" (pp. XXVIII-XXX).

a - Seven entries are wanting.

b - "A. Van Den BRANDEN" (nos. 4 and 19 on pp. XXVIII and XXII, respectively) is an error instead of "van den BRANDEN." Robin had already made the same error in his Bibliographie générale..., and it was denounced in Orientalia, l.c., p. 281. It does not seem very difficult to read "A. v. d. B." on p. VI of vdBrID, which is repeated in the author's publications of Philby's texts, and even more explicitely in p. 6 of vdBrID, viz. "A. van den BRANDEN" ("randen" in low caps).

c - The end of no. 19, "p. 172," must be corrected to pp. 172-175.

3 - Robin's diacritical signs.

The diacritical signs used by Robin in his transliterations of the texts (cf. already JaMAR, VIII, p. 25, and X, pp. 44-47) are most interesting. Let us take the first 30 texts as a testing ground.

In his thesis, RoHT, the reader finds the following signs:

. (= period) alone in RoHT 6 (p. 27), 13 (p. 47), 15 (p. 53), 16 (pp. 55-56), 17 (pp. 58-59), 22 (p. 64), 28 (p. 67) and 29 (p. 69);

• (= small circle) alone in RoHT 2 (p. 13);

? (= question mark) alone in RHHT 3 (p. 17) and 20 (p. 62);

. and ? in RoHT 7 (p. 39), 10 (p. 45), 11 (p. 46), 14 (p. 50) and 27 (p. 67);

• and ? in RHHT 4 (p. 20), 5 (p. 21) and 6 (p. 27);

[ ] (= brackets) used constantly without a single exception.

JaMAR, X, p. 45 has already stated that "the exact meaning of [first three] signs is not possible to guess;" cf. also l.c., XIV, p. 69. Now the wind has shifted; the weathercock has turned and points to another direction; viz.

• in pp. 114, 1[illegible]1, 122, 126, 127, 134, 141, 150, 151 and 152;

< > (= carets, [illegible]116, 119, 121, 122, 126, 127, 128, 129, 134, 138, [illegible]1, 151 and 152;

? in p. 126 (with *).

Nobody would ever dare to think that the preceding changes are not of earthshaking importance, of course, viz. [ ] and . have disappeared and ? appears only once, and the carets obviously replace [ ]. Unfortunately, the carets indicate that something was forgotten in the text, but Robin erroneously uses them to indicate his restorations of sections missing in the original because they were supposed to be on missing fragments. Furthermore, the preceding use of * cannot prove that . of RhHT had the same value as * for the simple reason that the wind has turned and with it - most probably - the value of the signs used there. Here again, Robin does not inform his reader either of his new set of diacritical signs or of the exact meaning he gives to any of them. Robin could of course hurl back that such question is of insignificant importance and, therefore, that it is not worthy of being fussed about. It should be emphasized here again that, since he does not explain himself at all, Robin could always argue that his signs were misunderstood. The whole situation may be summarized as follows. It is to be hoped that, when he publishes a paper, Robin writes to be understood; otherwise, he has no reason whatever for writing. Since the reader cannot find out the exact value of the gimmicks used by him because he steadfastly refuses to give the necessary explanations, he does not and cannot know what Robin means. Consequently, why does Robin write and/or keep writing at all?

4 - Ja 474/1-2 and 631/1.

An outstanding example of Robin's reasoning is the case of Ja 474/1-2 and 631/1, as seen in Sayhadica, p. 129. Robin writes in his footnote 11 that

"On s'étonnera que, dans l'édition de Ja 631, A. Jamme n'ait pas renvoyé à Ja 474 et corrigé ses erreurs de lecture et d'interprétation."

This footnote is appended to the following text:

"l'auteur du texte, *Qṭb*^{n} *'wkn bn Grt 'qwl s*^{2c}*<b*^{n} *S*^{1}*mhr*^{m} *Yhwld>*, est un qayl de *Dmr(y)*, comme l'indique explicitement Ja 474 de Na^{c}ḍ, qu'il faut lire:

1 *Q̇tb*^{n} *'<wkn bn Grt 'qwl s*^{2c}*=*

2 *b*^{h} *Dmrẏ<...* (11) =

The text involved is Ja 631.

a - It is immediately obvious how careless his transliteration system is. The dot immediately above n of bn (l. 2) must belong to that letter; in fact, it belongs to "ṭ" of qṭbn of l. 1!!! There is a small circle exactly halfway between "<" of l. 1 and "y" of l. 2; in fact, it belongs to the latter. - Finally, what is the value of "'" to the right of n of qṭbn (l. 1)?

b - His "explicitement," which is repeated on p. 127: "(lire Dmry et non Dmrt<...") is disproved by his own footnote 15 (p. 133)

"15 Voir aussi ci-dessus Ja 474 de Na^{c}ḍ, où on peut restituer Grt."

Of course, there is no contradiction between "explicitement" and "on peut" since the two expressions apparently are synonymous in Robin's language. Furthermore, his information on "de Na^{c}ḍ" (cf. also p. 127) remains unjustified. E. Rossi's manuscript does not contain any remark which could even remotely justify Robin's claim.

c - Even, his "on peut" is not corroborated by the original Ja 474 which is a fragment where the length of the lines remains unknown. Yet, in spite of such a fact, Robin fils the gap of the missing section with exactly 14 (neither 13 nor 15) letters composing an expression of which he does not give a single epigraphical example to prove that it really exists and is not the result of his concoctions!

d - Robin reads the initial letter of Ja 474 as "Q̇," the fifth as "'" (= ʾ), and the sixth of l.2 as "ẏ." His first reading is no more probable than "⌈y⌉" or "⌈h⌉;" his third reading is no more probable than "⌊t⌋," and the so-called certainty of his second reading is contradicted by the fact that, as already pointed out in the editio princeps, the letter could be "ʾ, b, d, etc."

In brief, my answer to Robin's footnote 11 is simple: my working method is based on the respect of the texts and originals as they are while that of Robin, who is imitating J. Pirenne's method, is primarily founded on his preconceived ideas and syntheses to which he accommodates the texts by brutally stretching them if and when necessary. This basic difference is clearly illustrated by the following expose. Robin needed his preceding distortion of Ja 474 to interpret dbn/dmr of Ja 631/29 as speaking "probablement pas de la cité mais de la tribu;" in fact, it is used as his second reason to prove his thesis. His first reason is as follows:" d-bn introduit d'ordinaire un nom de tribu " (p. 129). The truth of the matter is that his "d'ordinaire" is not only incorrect; it reflects precisely the opposite of the reality. Here follows the information presented in JaSIMB:

dbn + /s̆^{c}b (dual): Ja 577/11;
/s̆^{c}b (singular): Ja 644/16, 712/7; and
/ʾs̆^{c}b (plural) Ja 631/29;
dbn + a tribe name, hwln: Ja 616/23 and 635/33

} SIX (6) examples.

dbn + peoples: /ʾḥbān Ḥabašites: Ja 577/5;
/ʾsbʾ warriors: Ja 629/9;
/ʾsd soldiers: Ja 586/21-22;
/ʾʿrbn Arabs: Ja 561bis/11; 635/34;
/ʾfrs horsemen: Ja 576/4, 5, 8, 10, 12; 577/8;
/ʾqwl leaders: Ja 574/5; 575/3; 576/4, 5, 9, 10, 12, 15; 577/4, 8; 629/9;
/gyš troop: Ja 665/21;
/ḥms army: Ja 574/5; 576/4, 5, 6, 7, 8, 10, 12; 577/8; 629/10;
/mqtyt high military officers: Ja 577/7;
+ places: /ʾdwr Bedouin campings: Ja 574/8;
/ʾʿmd naturally watered fields: Ja 735/7;
/ʾrḍt lands: Ja 561bis/12, 14; 627/8;
+ name of a town: /ngrn Nagrān: Ja 635/33-34

FORTY (40) examples

Where is Robin's "d'ordinaire" with 6 cases out of 46? Indeed, the publication of the dictionary of his jargon should have been out in 1977 instead of his RoPH!

5 – The etymology of smhrm.

In Ṣayhadics (p. 116, footnote 1), Robin states that

"Le nom de tribu /s^1mhrm/ peut être analysé de deux manières, soit s^1m-h-rm ("Son nom est exalté"), soit S^1mhr^m (à comparer avec l'arabe Samhar dans al-Hamdānī Iklīl II, p.383, ou as-Samharī dans Ibn al-Kalbī/Caskel-II, à ce nom). Comme A.F.L. Beeston (1976, p.3 etc.), nous retenons la seconde, mais sans argument décisif."

a – Robin fails to inform his reader that Beeston does not justify his choice of etymology: he simply speaks of "Samhar" without even a short note, as if his etymology were evident as well as necessary. Thus, Robin's choice is nothing but sheepish.

b – Robin explicitely admits that he looked into the question but failed to find any conclusive element of solution in one way or the other.

c – Both Beeston and Robin have in common that they are motivated by their well-known pan-Ar attitude: it suffices for them to find "Samhar" in Ar sources to automatically transfer it into SA. This is the only excuse they have in support of their common attitude in the present case.

d – Both Beeston and Robin voluntarily sweep under the rug a well-known epigraphical fact, and this once more proves the preconception of their ideas, of their theories, and of their usual reasoning method which either utilizes the SA documentation inasmuch as it confirms their theories or distorts it to suit their ideas. The truth of the matter is that the etymology of smhrm as sm + h + rm (cf. already in RÉS, VII, p. 353, in RÉS 4763/2) is plainly justified by the numerous names of which the first two elements are sm + h. Independently from both smhrm and smhrym, JaSIMB (p. 422B) lists already six of them viz. smhʾmr, smhwtr, smhyfʿ, smhkrb, smhmʿ and smhʿly. Furthermore, that nominal derivation belongs to the first historical period of Sabaʾ and, therefore, precedes the Ar onomastics by well over ten centuries.

e – Robin gives an indisputable insight of the confusion which permeates his knowledge of onomastics in particular and, in general, of his askew argumenting method. He writes on the name smydʿ that

[A] "La forme même du nom, composé du
[B] verbe yd^o et du substantif s^1m (= s^1m-h)[3] signale un théophore où la divinité est désignée allusivement par l'expression "son nom"."

And footnote 3 (p. 120) reads as follows:

[Ca] "A priori, deux analyses de s^1m sont possibles: s^1m + pronom suffixe de la première personne singulier (*sim-ī) ou bien s^1m + pronom suffixe de la troisième personne singulier (*sim-hū/sim-hā). La seconde paraît plus probable dans la mesure où sont déjà attestés des noms de type s^1mh- dans lesquels le h disparaît de la graphie: voir S^1mkrb, nom d'éponyme, pour S^1mhkrb, dans Nami NʿC 1/7. On notera cependant qu'on n'a encore aucune attestation d'un nom "S^1mhyd^o." None of Robin's statements is acceptable. [Cb] [Cc] [Da] [Db] [E]

The very first criticism is that [A] is a manifest latius hoc because the same idea is presented in all certainty although the identical theory merely "paraît plus probable" in [Da], and this suggestion is erroneous and unjustified. [A] = [Co] is a deduction from [Db]. They are erroneous because it is impossible for Robin to prove that smkrb of NaNAG 1/7 actually is smhkrb, a name frequently attested in the Sab eponymic lists and contexts because they belong to two different nominal derivations. Indeed, A.G. Lundin had to admit that "ces personnes portent presque toujours les noms caractéristiques pour les éponymes" (cf. Le Muséon, 77 [1964], p. 412). Even without the preceding acknowledgment, it seems obvious that no assimilation or identification of persons is acceptable unless proven, not merely assumed to suit a preconcieved idea.

[B] also is erroneous. In names, such as smydʿ, there is no allusion, hidden or manifest, to a deity; and Robin invents the letter h which does not exist.

[Ca] also is erroneous. The name may safely be interpreted as "He is called connoisseur." The apocope of the final radical w is also attested in the verb hrḍ = hrḍw/hrḍy.

[Cb] is excluded by the fact that, here again, there is no way Robin can prove that the personal pronoun is understood. In fact, without spelling it out, Robin revives J. Ryckmans' erroneous theory on some nominal formations which JaCMYE (pp. 169–191) refuted at length, but Robin apparently does not care about that. [E] is a fact that has nothing to do with the name smyd^{c} because a name, such as unattested "smhyd^{c}," would be treated as another specimen of the same class as smhrm, thus a qtl + h + bdr, but smyd^{c} simply is a qtl + bdr derivation. The element bdr is used here to indicate that the second component of the name belongs to a root different from the first.

6 – Ja 3205.

RoDAA 4 = Ja 3205 was published by C. Robin in Raydān, 4 (1981) 1982, pp. 43–47, notes 1–6 on p. 62, and photo in pl.1, which is too dark and not sharp enough to allow a clear view of many letters. Robin's handling of the present text is another good example of erroneous reasonings under a seemingly scholarly cover.

a – The authors of the text.

Robin reads the first two names of l. 1 as "$.n^{c}m$ $'brr$" and his commentary flounders as follows:

(1)

[A] " l.1, $.n^{c}m$: avant le n, il y a peut-être place pour une lettre
[Ba] étroite telle que y. On restituerait volontiers $Yn^{c}m$ si le mo-
[Bb] nogramme de droite, formé avec les lettres de ce nom, ne comportait, en plus de n, ^{c} et m, une lettre avec socle (sans doute
[Bc] ' puisque b, s ou k ne donnent pas de nom attesté). Quant à la
[Ca] lecture $'n^{c}m$, suggérée par le monogramme, elle fait également
[Cb] problème car il ne semble pas y avoir place pour un ' au début
[Cc] du texte; en outre, ce ' ne peut pas se trouver à la fin
[D] d'une ligne antérieure car notre texte est certainement complet."

(a) The main basis of Robin's argument is his [D], viz. "notre texte est certainement complet," and this, in turn, forces him into an argumentation which proves that something is drastically wrong with it since either of his two hypotheses ends up in an impossibility.

(b)What Robin takes in his [A] and [Ca] as the possible remain of a letter is a section of the edge framing the text.

(c)The basis of the editor's argument is that the three monograms represent, respectively, the first name of l. 1, the name of the house (ll. 2–3) and the second name of l. 1, and that such an arrangement is a "distribution . habituelle" (p. 62, footnote 1). The editor takes "habitual" in the sense of "constant," which adds to the first the idea of invariability. That the repartition of the monograms as the editor describes is not unvarying is proven by SharaT 56 compared with Gr 27 (cf. below, III A 7).

(d)His [Ba] is an empty, useless babbling: the letter y cannot be read in the right monogram because the staff of the letter always is vertical; cf. Ga 3 and its photo in AION, 30 (1970), p. 1 (after p. 165), a text which the editor mentions in his footnote 1 of p. 62.

(e) His [Bb] also is erroneous. The basic form of b is (e.g., ll. 3 and 5) with a wide horizontal bar; this form is the right half of the lower section of the right monogram, which is composed of ^{c}, n, b and s, and this composition contradicts the editor's [Ca], [Cd] and [D], because the fourth letter belonging to "$.n^{c}m$" must be located on another stone. The normal reading of such a monogram is ^{c}nbm. This name is maintained by A. Avanzini in Ḥaḍr Yaṯūf 1 (cf. her Glossaire des inscriptions de l'Arabie du Sud 1950–1953, Firenze, II [1980], p. 100A) although I have already noted that "the drawing is obviously inaccurate, since the second letter, , is cut into two parts" (cf. RSO, 40 [1965], p. 188). Cf. the name ^{c}nbm in JaMAR, III, p. 7.

(2) On the second personal name, Robin states this:

"$'brr$: seule la première lettre est sûre; la deuxième peut être lue b, ', k, s, ṣ ou ṣ; les deux dernières sont à peine reconnaissables."

This is a good example illustrating once more the editor's workmanship. Note the gradual metamorphosis of "$'brr$" of the transliteration (p. 44) to "$'brr$" of the translation (p. 44) and, finally, to "$'brr$" on p. 45 (thrice)!

Philby's copy of "$'brr$" of Ph 113 (cf. JRAS, 1944, p. 124) cannot be trusted, and the facsimile of the copy is not even published in pl. 14. – The editor's preconceived idea of the left monogram representing the second personal name of l.1 influenced his reading of the latter to the extent that the second letter indeed

becomes "b̊" or simply "b" although the crucial upper half of the letter being destroyed, the letter could be ʾ, k, etc. The normal reading of the left monogram is the well-known name rʾb. Finally, the second letter of the name in l.1 cannot be "b" because it is composed only of one vertical staff as the next vertical staff to the left is connected to the specific triangle of d. Thus, the name may be read as ⌜(l)⌜d⌝ which may be completed as ʾ(l)⌜d⌝(n); cf. RÉS 3423A in JaMAR, VII, p. 130, note 60. Thus the beginning of the text may be read as follows:

2	.../w.?.	2	..., and .?.
1	nʿm/ʾ(l)⌜d⌝(n)⌜/d⌝ṣrf/bnw/šddyn	1	naʿim ʾA(l)d(ān), him of [the house] Ṣaraf, descendants of [the clan] Šaddādīyān.

The editor claims that

"d-Ṣrf et Šddyⁿ: l'auteur porte un double nom de lignage: le premier pourrait renvoyer à un bien et le second à un ancêtre éponyme."

This opinion may be discarded even at first sight for no parallel is given to justify such fancy an interpretation. Furthermore, in his translation (p. 44) and four times in his commentary (thrice in p. 45, and once in p. 47), the editor erroneously gives the family name of [.?.]nʿm/ʾ(l)d(n) as "d-Ṣrf." The letter d does not belong to the family name, but is the relative pronoun introducing the name ṣrf and relating it to the preceding name. This error is constantly repeated in Robin's publications.

3 - The central section of the text may be read as follows:

2	hqḥw/whqšbn/bythmw/dʾ	5	mw/rḥmnn/wkl/bhthw/wʾhw
3	swr/b//nṣr//rḥm//nn/w	6	thmw/ʾlt/mẓrn/(w)rhhw/dm
4	ḥmd/r//ḥb//wl///smʿnh	7	bkrn/ - - -

2 have prepared and established their house Dūʾas-
3 war with the assistance of Raḥmanān and
4 in praise of Rāḥib. And that may hear
5 Raḥmanān them and all his [=of the main author] valets and their
6 cousins, those of the (clan) Maẓurrān, during the (mo)nth of Mub-
7 kirān, - - -

L. 2: the editor translates hqḥw/whqšbn" as "ont aménagé et refait à neuf" (p.44), but, the commentary, which normally is tediously long, is reduced to ... nothing. The reader wonders why these two words would make an exception. In fact,

however, the editor disagrees with SSDS's translation of the verbs, respectively, as "complete satisfactorily, finish off "(p. 110) and "build, construct, make"(p. 108). But, at the time of the writing of his paper, Robin had too much to lose, had to secure his future and, therefore, could not afford to antagonize to "laudemus invicem" club members. The word byt is translated by the editor as "palais" (p. 44), which becomes "maison ou palais" (p. 45). The only detectable reason for justifying the first translation is that both words have the same meaning in Robin's vocabulary.

Ll. 3-4: wḥmd/rḥb is translated by Robin as "l'approbation générale(?)" (p. 44). Such an expression jars as much in SA phrasaeology as "un cheveu dans la soupe." The editor's illuminating commentary reads as follows:

"ḥmd pourrait avoir ici le sens de "approbation" comme dans Garb., Antichità yemenite, 9. Bayt al-Ašwal d/1 (...] b-ḥmd Rḥmnⁿ bᶜl smyⁿ) et nous verrions dans rḥb, attesté en sudarabique avec le sens de "largeur, ampleur" (CIH 541/109), un complément du nom de ḥmd avec valeur d'adjectif."

The expression bḥmd/rḥmnn of Ga 42/1 (cf. AION, 30 [1970], p. 546) is translated by G. Garbini as "a gloria del Misericordioso" and he states that "l'espressione iniziale bḥmd rḥmnn si ritrova all'inizio di CIH 541" (l.c.). This statement is erroneous: the beginning of CIH 541/1-2 is bḥyl/w[r]dʾ/wrḥ(2)mt/rḥmnn; it is in l. 93 of the same text that bḥmd/rḥmnn is found. The meaning of ḥmd is praise (as both substantive and verb); and hḥmd is "to behave in a praise-worthy way." The idea of "approbation" is foreign to the root ḥmd because it means "consent, assent." The expression bḥmd/rḥmnn of Ga 42/1 means "in praise of Raḥmanān." As for rḥb, it indeed means "thickness" in CIH 541/109-110 by apposition to ṭlm (l.107) length, and rym (l. 108) height. Even in the perspective of Robin's understanding, the concept of "largeur, ampleur" most certainly does not justify his grossly erroneous latius hos, viz. his translation as "générale(?)" (p. 44) unless, here again, "large" and "général" are synonymous in Robin's jargon. But, how could the reader be aware of such synonymity since his dictionary still remains unpublished?

Ll. 4-5: wl///smʿnhmw. The editor transcribes (cf. also p. 44) and comments on it follows: "l.4-5. w-l-ysmʿn-hmw: il semble que le lapicide ait omis le y puis ait légèrement regravé la pierre pour le suggérer" (p.46).

There is a faint word divider after the letter l. If the engraver had wanted to engrave y, he could easily have trimmed the right edge of the lower right section of the third monogram. There is no y there.

Ll. 4-6: the verb sm^{c} of l. 4 commands three complements, viz. -hmw (ll. 4-5), kl/bhthw/w^{2}hwthmw (ll. 5-5), and those three groups of persons are presented as being ^{2}lt/sprn. The whole pericope is translated by Robin as follows:

"Que les entende

5 Rhmn^{n}, dans toute Sa grandeur, eux et leurs frè-

6 res qui sont favorisés (par Lui)" (p. 44).

It should be emphasized that, in order to give some meaning to his rendering, Robin translates w before kl as "dans," and invents a whole expression "(par Lui)" at the end, an expression which should be between brackets, not between parentheses. Any recourse to such anomalies points to authors forcing their own ideas upon the original, and the interpretation suggested by them, being so basically vitiated from the very outset, does not deserve any consideration. On the word bht, Robin writes this:

[A] "bht-hw: le pronom suffixe renvoie à Rhmn^{n}: s'il s'était[sic!] agi de l'auteur du texte, on aurait eu -hmw (voir bxt-hmw, l.2;

[B] ysm^{c}n-hmw, l.4-5). Le mot bht est bien attesté en sudarabique: on le traduit par "grand" dans la mesure où il s'oppose à qtn,

[C] "petit" (voir par exemple CIH 619/2). Ici, c'est un substantif que nous rendons par "grandeur, majesté" faute de meilleure solution" (p. 46).

The word bht is well-known as member of an enumeration designating persons and, contrary to [C], it never has an abstract meaning. — The engraver was not very careful in his carving of the inscription because he chiseled "ʿ" instead of w of wrh, an error which Robin mentions in p. 47... but, the engraver did NOT attempt to correct his "ʿ" to w. It may thus be possible for the reader to surmise that the worker could also have made another engraving error, viz. bhtw instead of "bhthmw." Such a suggestion, however, is unnecessary because, if the first-listed author wanted to refer to himself, he obviously had to use the singular pronoun, contrary to Robin's [A] and his following statement:

"bnw: ce substantif est au pluriel, tout comme le verbe hqhw (l.2) et les pronoms suffixes de la suite du texte quand ils renvoient à l'auteur de l'inscription."

Note also the plural form of the relative pronoun ^{2}lt (l. 6). Incidentally, the reader also notes the singular of "l'auteur" referring to several persons!

In the present text, bht may also be translated as valets as in CIH 543/5 (cf. below, III D), and the following ^{2}hwt may be given the meaning of cousins (cf. JaSDMB, p. 427A) preferably to "allies" because of ^{2}lt/sprn of l. 6. The cousins of the authors certainly belong to the same clan, but not necessarily their "allies." The meaning of cousins is also suggested here because, were those persons "brothers" of the authors of the text, they would normally have been listed before bht.

sprn: in spite of his first opinion which is not presented as involving any kind of doubt, viz. "nous analysons sprn comme le participe passif" (p. 46), the editor notes four lines further down that

"Il n'est pas impossible cependant que ^{2}lt sprn soit le nom d'un lignage apparenté ou allié aux d-Srf."

Even his understanding of l. 5 cannot justify the analysis of sprn, which in turn does sustain the aberrant grammatical construction which he wishes to find in the text. The obvious interpretation is that sprn is the name of a large ethnic entity of which srf is a part. Cf. Ar sarra "to split" (cf. LaAKL, p. 1909A), and the sqtln nominal derivation, e.g., as in sdbn of RÉS 3946/6.

Ll. 6-7: sbkrn: here again, as he has done with srf (l. 1), Robin erroneously considers dsbkrn as the name of a month; cf. "d-Sbkr^{n}: ce mois" (p. 47)(cf. already above, p. 54, for another example of the same error). The name is sbkrn (e.g., Dostal 1/8). There is not a single name of month which begins with d (cf. already above, p. 7); cf., e.g., JaSDMB, p. 418; otherwise, expressions, such as bwrh/d^{2}bhy (Ja 735/4) in the month of ^{2}Abhay, or lbdr/^{2}bhy until [the month] ^{2}Abhay arrives (Ja 651/17), would be erroneous; viz. bwrh of the first expression and ^{2}bhy of the second should have been "bwrhn" (which is not attested) and "d-^{2}bhy," respectively. This is a basic and simple epigraphical fact, but Robin does not wish to see it and prefer to keep his eyes open in another direction, viz. to follow the flock, from G. Ryckmans in Les noms propres sud-sémitiques (Louvain, I, pp. 378-381, where all the names of month begin with d) to A.F.L. Beeston who still refuses to accept the epigraphical evidence in BeSC, p. 37, par 19:16, where he speaks of "(month) Dū Di^{2}āwān" of Proche-Orient 1/10: wrhn/dd^{2}wn which G.

Ryckmans translated as "au mois de Dû-Ba'ân" (cf. Le Muséon, 67 [1954], pp.100 and 101, respectively). Moreover, Robin's translation of mbkrn as "mai" is erroneous for mbkrn being a proper name is untranslatable regardless of what Robin's theories on the subject might be.

7 - Gr 27.

The idea of "étages," which Pirenne erroneously claims to have found in RES 3965 (cf. above, p. 20) is also put forward, with the same result, by Robin in his republication of Gr 27 in Sayhadica, pp. 151-152 and photo in p. 158. Although the word which Robin translates as "étages" is an hapax legomenon, bhwr, his commentary does not contain a single word to justify his rendering. Of course, it is much easier to juggle with theories than to do a basic, honest groundwork. Indeed, the reader is authorized to here again turn against Robin his own remark on M.A. al-Ǧaryânî's opinion on 'rb'w of Ir 40/1-2, viz. "dont on aimerait connaître les arguments" (cf. Sayhadica, p. 118).

Contrary to what he does with Sharafaddîn's publication, Robin does not list Gryaznevich's volume in either his text or his bibliography (p. 153). Contrary to what he does with G. Garbini's paper which he does not at all use in his text, but of which he gives the reference in his palaeographical remarks, Robin does not give in his text any information on Gryaznevich's publication. Robin's attitude is all the more surprising in that his whole commentary is strictly limited to redress Gryaznevich's misreadings. Contrarily, J. Ryckmans's review of P.A. Gryaznevich's volume has an entry in Robin's bibliography (p. 155) although it contains less than three insignificant lines, which read as follows: " Gr. 27

(p. 37 sq). nous enseignant puisqu'il mentionne une destruction (par le feu? dhr) [sic!] par les Éthiopiens à Zhafâr, et par le fait, jusqu'ici sans parallèle, que le texte est encore païen, bien qu'il remonte au Ve siècle " (cf. Le Muséon, 92 [1979], p. 206).

For the meaning of dhr, cf. below. Ryckmans' justification of his dating is wanting: the slenderness of the letters suggests the first half of the 4th century A.D.; cf. JaSDKB, pl. J. Here is the necessary information: P.A. Gryaznevich, Yujnaya Araviya. Pamyatniki drevnei istorii i kulturi (Akademiya Nawk), Moscow, Part-I, 1978, p. 37 and photo in pl. 44 in p. 142B.

Gr 27 is a Sab text in relief with the left section still absent; I have studied the original on Sunday, November 10, 1974.

a - Robin's erroneous identification.

Robin claims that the text was seen by A. Sharafaddîn and that its photo is published in the latter's Ta'rîḫ al-yaman at-taqâfî, Cairo, II, 1387/1967, fig.56, p. 70. This identification is erroneous. Sharafaddîn' caption of the photo presents the contents of the inscription as follows:

"وجدت بيع وبنيو شرحبيل ودوم و..... بنو اميكلامن أهل نشر وعثر الخارق ....."

Unfortunately, the photo is very poor and mostly illegible. Yet, in spite of the obvious similarities between SharafT 56 and the present text, the two inscriptions are different because of the following reasons.

(1) The initial monogram of Gr 27 is in the third place in SharafT 56; the second monogram of Gr 27 is the first in SharafT 56. For the importance of the preceding fact, cf. above, p. 53.

(2) The first symbol of SharafT 56 occupies the height of the three lower lines of the text only as the second monogram of Gr 27 occupies the upper three lines of the text only.

(3) The beginning of ll. 2-4 of Gr 27 cannot be read in ll. 2-4 of SharafT 56, beginning.

b - Details omitted by Robin.

The grayish sandstone is reused 1.80 m above ground level in the southern wall of the house of aš-Šayḫ Ḥamîd Ḥizzân al-'Awwal, which is located in the upper section of the town.

c - Corrections to Robin's museographic description of the stone.

The measurements of the stone are 98.5 x 28.5 cm. Letter height: 4.5 (l. 1) and 5.5 cm (l. 4).

d - Robin states that the palaeography of Gr 27 is "comparable à celle de" Ga 5 of which he reproduces a good, but unfortunately too small, photo in p. 159B. In his vocabulary, "comparable" means identical. Yet, the main difference is w the two sections of which are much less imbricated than in Ga 5 and even very close to being contiguous to one another. Cf. also above, the remark on Ryckmans. Furthermore, Robin would have been well inspired in cutting off the reproduction of the antlered bucranium which is close to CoAZ 81 (cf. AIOM, 33 [1973], p. 198, and photo in pl. 20B) and has nothing to do with the contiguous text; the advantage of such a cutting would have been welcome, viz. a larger photo of Ga 5.

e - Translation of the text.

L. 2: the verb dhr is translated as "incendier" (cf. also above, Ryckmans). This is erroneous; the verb dhr means to conquer (cf. also Ja 1028/3). Any connotation with "fire" is foreign to the root. A.F.L. Beeston claims that "the appropriate rendering of the verb is thus 'destroy,' or more specifically 'burn' (this being the fate of the church in the traditions)" (cf. BSOAS, 1985, p. 42).

and he translates dhr of both Ja 1028/3 and Ḥimā/3 as "destroyed" (p. 45 and 49), a translation which is not supported by Ar. This method of argumenting has already been rejected in JaMAR, XIV, pp. 109-110. It does not seem difficult to understand that accounts of later traditions, even if and when they are accurate, do not necessarily contain the translation of words used in the inscriptions. Note also that pp. 110-117 of the same JaMAR, XIV, also discussed beforehand and rejected Beeston's interpretation of "The Chain of al-Mandab" (pp. 50-51) and of the testimony of the "Martyrium S. Arethae" and Ibn al-Muǧāwir. Incidentally, it is difficult to understand why Beeston in his footnote 19 does not refer to the revised, annotated and paragraphed text of Acta Sanctorum, XXIV, Octobri 1869, pp. 748foll, and to the 1962 reprint of Anecdota Graeca.

L. 3: the only difficulty of the text is the hapax legomenon bḥwr, the internal plural of bḥr in tstt/bḥwrm, which is a modal adverbial clause apposed to bnwt. Robin translates the expression bnwtm/tstt/bḥwrm as " un édifice de six étages." Here again, the reader can turn against Robin his criticism of M.A. al-ʾIryānī (cf. above, p. 58) because his commentary does not speak of the word bḥwr and, therefore, does not justify his translation. In fact, the word implies a situation exactly the opposite of what Robin claims it to be. The root bḥr always implies the concept of "wideness, spaciousness;" e.g., baḥrat " wide tract of land" (cf., e.g., LaAEL, pp. 1568-1578). Therefore, bḥwr describes the building horizontally, not vertically. The expression means "a building composed of six contiguous wings" or, more simply, "a building of six contiguous wings." That those wings were contiguous is indicated by the singular bnwt.

L. 3 - The word byt is erroneously translated as "palais (cf. already above, p. 55). Were it a "palais," it would surely have been listed before bnwtM A building. It being listed in the second place certainly suggests that it was smaller than the first with its six wings. It was a simple house.

8 - RÉS 4708 A and B.

Also in Ṣayhadica (pp. 134-135), Robin republishes RÉS 4708A, an inscription engraved across the chest of each of the bronze monumental statues of the kings Damarʿalay Yuhabirr and his son Taʾrān on the basis of two copies. He writes that the text " se lit, si on se fonde sur les copies publiées par les restaurateurs " (p. 134).

His translation reads as follows (pp. 134-135):

" Traduction:

1 Dmrʿly Yhbr et son fils Tʾrn, rois de Saba' et de ḏū-Raydān, ont érigé ce qu'ils accordèrent à leurs sujets

2 Bkl ʾḥrr, bʾrḥbʾmyd et Mqdm benu Ḏrḥ pour la salle d'audience de leur palais Sḥn "

Robin speaks of course of K. Weidemann's pamphlet entitled Könige aus dem Yemen. Zwei spätantike Bronzestatuen (Mainz, 1983, 31 pg.). Robin's statements call for the following remarks.

a - The very first remark to be made is that, by simply referring to "RES 4708" instead of to RÉS 4708A, Robin pays no attention to the second text engraved on the statue, B, viz.

lḥyʿn	1	Laḥayʿamm
hnsg	2	has composed [it].

For the photo, cf. M.A. al-ʾIryānī in al-ʾIklīl, II, 2-3 (1403/1983), p. 272, bottom left. The text had been announced by W.W. Müller as "Laḥayʿamm and the verb hnsg ... put together the parts of the statue" without any reference to its division into two lines (cf. PSAS, 9 [1979], p. 79). It is very interesting to note that even before the publication of the text itself or of any photo the verbal form hnsg was incorporated in SDDS, p. 99. It was honorable of J.C. Biella not to take up Müller's newspaper-like information in her Dictionary... although her bibliography lists Raydān, II (1979), four times (p. 551, 553, twice, and 560).

b - Here again and in a spectacular way, Robin contradicts himself openly because he emphatically stated in his 1977 thesis, RoPH, p. 390, and repeated verbatim in his 1982 publication, RoHT, II, p. 90, a phrase which has be reproduced here again (cf. already above, p. 41), viz. "une publication d'inscription sans photographie est inutile." JaMAR, XIV, already denounced his illogicality (pp. 271, 286, etc). Cf. below, III A 9, for another example of the same inconsistency.

c - Robin does not explain or justify why he did not take advantage of one of his trips to Ṣanʿāʾ to check those copies against the originals, an initiative which was all the more necessary in that, according to him, one letter "pose un problème" (cf. footnote 17 in p. 134); cf. below. Neither does he explain why the French mission in Ṣanʿāʾ apparently did not take any closeup photos of the texts since the two statues were returned to Ṣanʿāʾ many years ago.

d - The photographic documentation published prior to Weidemann's

pamphlet was not good enough to ascertain the complete reading of the texts and the complete palaeographical study of all letter forms; cf. M.A. ʾal-ʾIryānī in al-ʾIklīl XI, 2-3, pp. 270-272. The best photo published was P. Costa's (cf. AION. 34 [1974], pl. 68 after p. 290): the photo is clear, large so that at least a small section of the text engraved on Ta'rān's chest can be studied at ease; it shows the following sections: /yhbr/wmhw
l/'hdr/wšr.

This photo is reproduced below. The above-mentioned paper of Costa is n o t referred to by Robin in either his text or his bibliography. The only Costa's publication used by Robin is his booklet, The pre-Islamic Antiquities at the Yemen National Museum, Rome, 1978, pp. 38 and 39, where Robin copied (p. 134, footnote 16) the sigla "YM 306 = Costa 73" and "YM 196 = Costa 74."

But, Robin's spectacular blunder is not limited to Costa's AION paper; it also involves Weidemann's documentation. When he speaks of "copies publiées par les restaurateurs," he doubtlessly has in mind the two tracings of Weidemann's p. 19. But, the color photo of p. 17 clearly shows the whole text on Ta'rān's chest. Therefore, his whole note "si on se fonde sur les copies publiées par les restaurateurs" simply proves that he uses the documentation he chooses ... in flagrant contradiction with his repeated statement on "une publication d'inscription sans photographie est inutile." Weidemann's photo in p. 17 proves that his copy of p. 19A is accurate and reliable.

e – Another important characteristic of Robin's study is his systematic omission of any reference to prior publications of JaPS and of Beeston.

(1) In his footnote 18 (p. 135), Robin refers to "Ja 493 = RES 4708/2." First, his "493" is an error instead of 491. Apart from this correction, Robin does not allude in the least to JaPS's remarks which, already in 1954, oppose his general interpretation of the text.

(2) In 1978, A.F.L. Beeston's translation of the text reads as follows (cf. Le Muséon, 91 [1978], pp. 196 and 209, respectively):

"two kings set up what they [sic!] gave (dgnwy) to their vassals X Y Z of the Bani Dhurah for the ma'sal of their house, the apartment which was thus adorned was certainly the guest-hall or reception room where visitors were entertained" and

"*Addendum to p. 196.* An improved text of R 4708, with the reading dgnwy, is available now that the statues have been cleaned."

Thus, according to Beeston, the two kings not only set up but also gave their own commental bronze statues to be on display undoubtedly as by far the best specimens on exhibit, in the reception room of the house of three of their "vassals." What a fantastic story! It is unfortunate that Beeston, who always is so prolific in illustrating his theories with foreign so-called parallels or examples, did not relate a similar case from somewhere else in the world. It is obvious that he did neither read nor heed, or both, some of JaPS's remarks. Furthermore, Beeston gives another proof of the inaccuracy with which he conducts his studies by giving the title of P. Costa's paper as " *Note sul alcuni pezzi* " (p. 196, footnote 3). The actual title is "Note su alcuni pezzi del Museo Nazionale dello Yemen a Ṣan'a," which is the paper mentioned above, in d of p. 62. Moreover, Robin does not refer to Beeston's study in either his text or his bibliography (p. 153). Yet, his translation of the text is practically the same as Beeston's! Finally, Weidemann's summary of the inscriptions also is practically the same as Beeston's: it reads as follows (p. 16): the texts

" aussagen, daß der König Damar alay Yuhabir und sein Sohn Tharan diese Figuren drei Männern aus der Sippe Darnath gestiftet haben für die Empfangsräume ihres Palastes."

And Weidemann does neither indicate where he copied his summary nor list Beeston in his bibliography (p. 31).

Robin states on al-ʾIryānī's reading (p. 255A) that "sa transcription comporte six nouvelles fautes" (p. 135). In fact, the so-called sixth error is the letter which Robin himself misreads (cf. below).

f – Remarks on the text itself.

(1) ḫmy (l. 1): Robin endorses one of RES's translations (p. 136); cf. already Beeston's "set up." Already in 1954, JaPS stated this: "ont placé [cf. also Gat 874], c'est-à-dire ont inauguré" (p. 326) and, consequently, the verb is translated as "ont inauguré" (p. 324). The mere setting of the two statues in place was not a matter of concern for the two kings, but for the three courtiers, but their inauguration, which such oversized statues certainly deserve, was an honor due to the kings.

(2) ḥmry (l. 1) is the most important word because the general meaning of the inscription, including the actual meaning of the verb itself, depends on the interpretation of the dual of the verb.

According to both Beeston and Robin, the subject of ḥmry is the two kings. The most obvious objection was already stated in JaPS (p. 325) as follows: "On ne comprend pas aisément la raison pour laquelle ces deux rois auraient agi de la

sorte en faveur de trois de leurs sujets; car c'est plutôt le contraire qui se produit ordinairement." These three persons present themselves simply as bny/ drnḥ, not even as chieftains or any kind of officials of the clan drnḥ; and their relation with the two kings was that they were their m³dbt courtiers. It would, indeed, be an extraordinary event for the two kings to offer to three of their courtiers two monumental statues of themselves. Such a thing does not happen, but the reverse does.

Another case of the same feature, plural subject and a dual verb is CIH 581 of which Beeston's study (cf. Arabian Studies, 4 [1978], pp. 21-25) was detailedly rejected in JaMAR, XII, pp. 17-28; in CIH 581, three women ḥqnyy.

(3) m³dbt (l. 1) is translated by Robin as "sujets." He confuses ³dm (cf. already above, p. 15) and m³dbt; cf. already JaIMS, p. 326. Furthermore, Robin fails to mention that SDDS has this :"n.p.m³dbt . vassal"(p. 1); thus, a plural (cf."n.p." = plural noun on p. XVII) is translated as a singular! Furthermore, SDDS also translates ³dm as "vassals" (p. 2; fortunately in the plural); thus, SDDS, too, confuses ³dm and m³dbt! Moreover, there is no SA epigraphical evidence of vassalage. Finally, JaIMS discusses the etymology of m³dbt and concludes that "le terme m³db [singular of m³dbt] peut donc désigner un commensal, un ami, un familier reçu à la table de quelqu'un, et pourrait être traduit, dans le présent texte, par 'courtisan'" (p. 326). Cf. also below, III A 9, Ja 3199/2.

(4) ³ḥḏr (l. 2).

The second name of the first author of the text is, according to Robin, one of al-³Iryānī's "six nouvelles fautes" (cf. above, p. 63). He deals with that letter in his 7 1/2 line footnote 17 (p. 134), which is worth reproducing here:

" La lecture de la troisième lettre de ce nom de personne épithète pose un problème. Le caractère, le seul de cette forme dans tout le texte, ne se distingue guère d'une barre de séparation: il est seulement un peu plus large et comporte deux traits horizontaux au quart et aux trois quarts de la hauteur. Il ne peut pas être lu *mn* (comme le proposait le RES) car cette lettre est caractérisée par deux petits triangles, en haut et en bas. M. al-Iryānī lit un *ḏḏḏ* et transcrit *ʾḥḏr*. Comme cette épithète n'est pas attestée, il semble préférable de lire, au moins provisoirement, un *tʾ*, ce qui donne *ʾḥtr*, épithète bien connue. Ni le *tʾ*, ni le *ḏḏḏ*, ne se trouvent par ailleurs dans le texte."

To make it obvious how ridiculous Robin's note is, here is reproduced Costa's photo as it is in AION, 34 (1974), pl. 6B (after p. 290):

The letter could hardly be clearer; therefore, who needs Robin's description of it? It is even ridiculous to point out that the letter "ne peut être lu *mn* (comme le proposait RES)" as if RÉS had made a mistake. What is the purpose of repeating it on p. 135: "Le texte du RES, d'après une copie de C. Ansaldi, comportait plusieurs erreurs: l. 1, *mḥmry* (au lieu de *d-ḥmry*); l. 2, *ʾḥmr* (au lieu de *ʾḥṯr*)," and, in the process Robin repeats his own gross error! But, there is a second contradiction between Robin's footnote 17 (p. 134) and his text of p. 135. In the latter, Robin characterizes al-³Iryānī's reading of ³ḥḏr as one of his "fautes." But, in his footnote 17, he does not say so; instead, he rejects al-³Iryānī's reading because of an irrational reason, viz. "comme cette épithète n'est pas attestée, il semble préférable de lire, au moins provisoirement, un *tʾ*, ce qui donne *ʾḥtr*, épithète bien connue." Who can accept such a gibberish argumentation? Furthermore, there is nothing "provisoire" about his reading; it is manifestly dead wrong. Finally, such a reading is priceless coming, as it is, from Robin who, after brandishing the gaudy banner of "une publication d'inscription sans photographie est inutile," sweeps under the rug the clear photo not only of one but of two authors so that he may write his preposterous footnote 17.

(5) mśwd (l. 2) is translated as "salle d'audience" by Robin. What a pompously dramatic rendering, copied from SDDS (p. 139), of a word belonging to an ordinary house! No wonder, then, that he translates byt as "palais." It is also something like a consolation to see that SDDS did not retain as a possibility J. Ryckman's interpretation of "terrasse (? mśwd)" (cf. BiOr, 12

[1955], p. 308B). SDDS's translation, which endorses Beeston's ideas, was not new at all; the meaning was already known in Min texts. But, contrary to Beeston (cf. Le Muséon, 91 [1978], p. 195), the translation of the word as "incense-burner sanctuary," which is maintained here, was well-known before 1962; e.g., N. Rhodokanakis (cf. Studien zur Lexikographie und Grammatik des Altsüdarabischen, Vienna, II, [1917], p. 34: mśwr "Brandopferplätze"), and who would dream that those ancient people would burn, e.g., animals, in their own houses? It is there that those people would normally place their incense-burners, so many examples of which have already been found; cf. JaIMS: "mśwd peut désigner non seulement l'autel où l'on brûlait les parfums, mais aussi le sanctuaire où ces offrandes étaient consumées [] Dans un pays aussi fertile en aromates que l'Arabie du Sud [], ces autels devaient être assez nombreux, même dans les sanctuaires familiaux; le nombre important des exemplaires trouvés jusqu'à ce jour, semble confirmer cette interprétation" (p.327).

(6) bythmw (l. 2): Robin notes that

" Contrairement à Maria Höfner (Sab 63 = RES 4708), nous estimons que "leur palais" signifie "le palais des banu Drnḥ" et non "le palais des rois". Le pronom suffixe -hmw (et non -hmy) est un premier argument. "

First, it is an error to relate -hmw to drnḥ, the clan affiliation specified by bny/drnḥ, an expression which is a mere part of the identification card of the three authors and has nothing to do with any of the latter's activities. Secondly, since Robin refers to a correction published in JaIMS (p. 326), the reader is authorized to assume that he also read the rest of the paper. Then, it is plain dishonesty for him to write his above-quoted statement without mentioning that such was already JaIMS's interpretation which is clearly expressed as follows: "la résidence occupée par les trois courtisans" (p. 327; cf. also p. 328). Furthermore, since Beeston does not elaborate on his translation of bythmw, it is impossible to know the accurate meaning of his "their house."

Robin continues (p. 135): " Un second est que rien ne permet de supposer que an-Nahla al-hamrâ' ... soit une ville royale ḥimyarite. "

This his second argument misses the mark because a "palais" is not necessarily the domicile. Furthermore, here again, had Robin cared to read M. Höfner's commentary to the end - after all, it is only a little more than 22 short lines long - he would have seen - it is to be hoped - how simplistic and irrelevant his second argument really is. Höfner speaks of "eine Summeresidenz der sab. Könige" (cf. Sabaeica, Hamburg [1966], pp. 29B and 31A). Indeed, nobody has ever suggested that the royal residence was at an-Nahla al-Ḥamrâ' or that this town had ever been "une ville royale."

(7) Robin crowns his study by presenting his summary of the text (p. 135): " Le sens du texte ne pose guère de problème: les deux rois ont fait don de deux statues afin qu'elles ornent la salle d'audience du palais Ṣ°. propriété de darânihides. "

Without apparently being aware of it, Robin introduces in his summary a detail which contradicts one part of his commentary, viz. "le palais des banu Drnḥ" (cf. above, p. 66) becomes in his summary "propriété de darânihides." As it stands, the last expression means "property of some persons of drnḥ" ... and his theory on -hmw of bythmw is contradicted.

g - Translation of the pericope.

The above-mentioned pericope may be translated as follows:

The two kings have inaugurated what had provided the courtiers of them both, [viz. three individuals, who are] of the descendants of Darnaṇ, for the incense sanctuary of their own house.

According to this interpretation, the three courtisans are responsible for both the statues and the text. The statues "ont été faites pour représenter les deux rois et les unir plus intimement au culte familial des trois personnages" as a special act of captatio benevolentiae "dans la résidence des trois courtisans" (cf. JaIMS, pp. 326 and 328, respectively). The two kings, having accepted their homage, inaugurated the statues. As a further act of captatio benevolentiae, the three courtiers drew the text with its main sentence commemorating the kings' act instead of putting the emphasis on their own initial act of deference toward their kings and the purpose of their initiative.

Additional note.

In his footnote 19 (p. 135), Robin refers to al-Scheiba's publication (cf. Die Ortsnamen in den altsüdarabischen Inschriften (mit dem Versuch ihrer Identifizierung und Lokalisierung), Marburg 1982. I wish to take advantage of this opportunity to make the following remark. The case of ʿwfm and ʿdbh of Ja 576/2 and 11 includes the erroneous reasons and argumentations offered by J. Pirenne, A.F.L. Beeston, G. and J. Ryckmans which were silenced once and for all by JaMAR, X, pp. 29-30 and photo of the center of the text in pl. 5. The performance of those four authors was reiterated by al-Scheiba who simply states "YGRN Ja 576/6 (., A.Jamme liest ylrn), ... mnqln/dygrn" (p. 155). Of course, for the "laudemus invicem" club members and their affiliates, a reading from the original has no importance. So, al-Scheiba corrects my reading without giving any comment or justification,

Yet, he is so positively certain that his correction IS the right reading that the original ylrn has no entry on p. 159. al-Scheiba's correction is all the more irresponsible in that JaSDM (p. 73A) explicitely states this: "dylrn (not dygrn as in [the edition of the text]; the commentary on p. 158 is erroneous)." al-Scheiba's is a gross error, similar to Robin's "ṯ" instead of ʾḥḍr. Note also that in his bibliography (p. 155, no. 28), Robin misread "Hassan" as "Hasan."

9 - Ja 3199.

In Raydān, 4 (1981), pp. 84-85, C. Robin - M.A. Bâfaqîh publish a little more than one third of a rock inscription under the siglum "Nāǧī 1."

a - The origin of the copy.

The initial paragraph of the paper reads as follows:

" Ahmad Nāǧī Sārī, Directeur du Musée national de Sanʿā', a copié toute une série de graffites rupestres à Hammat ad-Dubaʿ. Il les a confié [sic] pour étude à Muhammad Bâfaqîh."

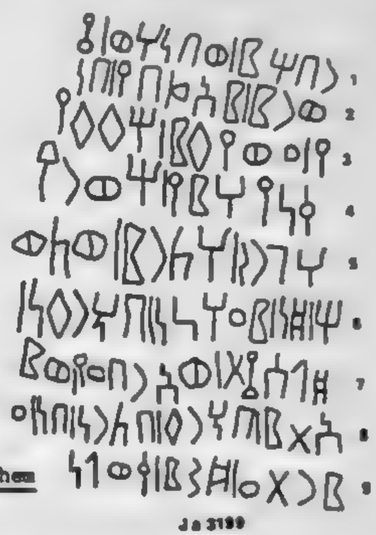
Since I did not witness the meeting during which the transaction was made, I do not know what was said. Nevertheless, the word "étude" is not synonymous of "publication." Furthermore, in several of his trips, Nājī was my wage-earning traveling companion and as such, he had, and still now has, no right to dispose as he wishes of the copies which I let him take. Moreover, I was the one who called his attention to the date of the text; showed him the exact place; and read it to him; therefore, the published extract is my own decipherment of the inscription. Since I have never terminated that contract and have no intention of doing so, my exclusive right of publication has never been, and will never be during my lifetime, canceled. Therefore, any further publication of Nājī's copies by anyone but myself will be denounced as a deliberate breach of professional ethics.

b - The coeditors' publication.

Here again (cf. already above, p. 61), although Robin himself had made his position known in most striking words in 1979 as follows: "une publication d'inscription sans photographie est inutile," a statement which he repeated verbatim in 1982, he publishes a text without any photo; not even a rough facsimile is added to the paper. Robin's statement is all the more hypocritical in that he, himself, did already publish texts without any photograph (cf. already JaMAR, XIV, pp. 271, 286, etc) OR republish RÉS 4708A (cf. above, pp. 60fol) while sweeping under the rug t w o published photos, the second being in the very same pamphlet where he read the copy of the text.

c - The text. It was on Saturday, January 18, 1975, on my way back from a visit to J. Maʿsâl that I stopped about ¾ klm northeast of Sanabân at a hill called Ḫammat aḍ-Ḍabaʿ. My notebook has "Sanabân" (cf. also P.A. Gryaznevich, Yuzna-ya [cf. above, p. 58], p. 120, map 9) while Robin spells it "Sinabân." I discovered the present inscription about 75 m above ground level and engraved on a vertical side facing the northwest. Text: total height: 22.5 cm; length of the lines: 11.5 (l. 1), 15.5 (l. 6) and 16.5 (l. 8) cm. All eleven m's and a single n's (l. 5) out of eleven are dextrograde. The text, which is published here as Ja 3199, may be read as follows:

1 rbḥm/wbnhw/t
2 wrm/mʾdby/bn
3 y/ʿwyfm/ḥffy
4 qnyhmy/ḥwry
5 hgrn/hkrm/wkw
6 h/dn/mʿhnn/bḥrfn/
7 dslstt/wʾrbʿy/wm
8 ʾtm/bhrf/hkrn/bn/ʿ
9 mrtʿ/dšm/qwln


Ja 3199

1 Rabḥum and his son Ta-
2 wrum, the two squires of the
3 clan ʿUwayfum, have protected
4 the slaves of them both. The two of them
were residents of
5 the city Hakarum, and have gouged
6 this permanent (inscription) during the year
7 one hundred and fourty-six
8 of the year of Bakrân, son of ʿA-
9 mmrataʿ, him of (the clan) Šaya, the ruler.

L. 1: rbḥm, e.g., Ḫaḍr Ja 2491e, Qat RÉS 3871/1, and Sab Ja 2912 a/2-3.

Ll. 1-2: tvrm, e.g., Ḫaḍr Ja 937/2, Qat Ja 1351, and Sab Ja 2860a.

Ll. 2-3: mʾdb, cf. above, p. 64 ; a synonym of "courtier," such as squire, fits the text better. The expression mʾdb/bny/ʿwyfm of the present text is paralleled by similar expressions in the same collection of rock inscriptions; e.g., mʾdb/bn/mšḥmm squire of the clan Mušḥamum, and mʾdb/bny/šḥkm squire of the clan Šaḥkum.

L. 3: ʿwyfm, cf. the Qat personal name in Ja 208/1. - ḥffy, dual of ḥff; cf. Ar

ḥaffa, 1st and 2nd forms "to go round about, circuit, compass, surround" (cf. LaAEL, p. 597A) and Hebrew ḥâfaf, e.g. "beschirmen" (cf. BuWG, p. 249B). It is not possible to even remotely figure out what the verb refers to, but the more common meaning in Ar, "to shave,depilate," does not seem very probable.

L. 4: ḥwry, dual of the verb ḥwr. In his treatment of ḥwr, Robin gives another example of his revolting reasoning method. His treatment of the word is found in a paragraph of 25 1/4 lines of RoHT, I, p. 88, and notes 235-237 on p. 141 = RoPH, pp. 118-119 and notes 22 and 23 on p. 460. The two texts, however, are far from being identical as shown by the following schema.

RoPH	RoHT, I
p. 118, l.12-p. 119, l.6A	p. 88, ll. 1-21A
—	ll. 12B-31A
p. 119, ll. 6B-9A	ll. 31B-34
ll. 9B-12A	—
l. 12B-16	p. 89, ll. 1-6
p. 118, ll. 12 and 22: "ville"	p. 88, ll. 1 and 11: "cité"
l. 23: "tribu"	l. 12: "confédération"
l. 25: "..."	l. 14: "etc."
p. 119, l. 3: "que je traduirais"	l. 18: "qu'on traduira"
l. 4: "Šabw[a)"	l. 19: "Šabw[a')"
l. 6: "Anān" [sic!]	l. 21: "ʿAnān" [sic!]
"en opposition à" [sic!]	l. 31: "en opposition avec"
l. 7: "simples habitants"	—
"j'ai l'impression"	ll. 31-32: "il semble"
"les"	l. 32: "de"
l. 8: —	"de la cité"
l. 9: "leur ville"	l. 33: "la cité"
p. 460, note 22, l. 2: "je crois"	p. 141, note 235, ll. 1-2: "nous croyons"
note 23, l. 1: "je prends"	note 236, ll.1-2:"nous prenons"
l. 7: "me paraît"	l. 8: "paraît"

(1) Robin speaks only of the plural construct "ḥwrw" ... as if the singular did not exist; cf., e.g., Ja 727/3. In the "INDEX DES INSCRIPTIONS INEDITES ET DES TERMES REINTERPRETES" of RoPH (pp. 574-616), the noun ḥwr ou ḥwrw is not listed (p. 592). In the "INDEX DES MOTS SUDARABIQUES DEFINIS OU REEXAMINES" of RoHT, I (pp. 183-184), the reader finds on p. 183: "ḥwr(w)-, 'habitants'." Thus, there is no singular.

(2) After repeating what has been known for a long time that ḥwr/ḥwrw actually are inhabitant(s), it seems that he should have mentioned another word having a similar meaning, hgr. The difference between the two nouns is that the first always is followed by either hgrn/X or a pronoun referring to a town. Therefore,

ḥwr does not necessarily mean townspeople; it gets that meaning from the genitive; consequently, ḥwr could be found with another genitive. Conversely, hgr cannot but mean townspeople because hgr means town; and the noun is used generally, viz. without specifying to which town the persons involved belong.

(3) Robin then adds "quel que soit leur statut social." This is erroneous because ḥwr is never said of a slave who has no right whatsoever. But, Robin contradicts himself in his note 236 (p. 141) where he writes this:

> "un esclave peut être ḥwr (Ḥūr Rūrī 1 etc.); le ḥwr n'est donc pas nécessairement libre. De plus des ḥwrw- se disent tout naturellement ʾdm du lignage qui domine localement (CIH 102 etc.): ces deux termes ne s'opposent nullement."

For his opinion on ʿbd, cf. JaMAR, XIV, p. 244. His contradiction comes from the fact that he opposes ʿbd and ʾdm but, as pointed out by JaMAR (l.c.), "Robin does not even allude to the fact that ʾdm and its variants serve as the plural of ʿbd" (cf. already Cahiers de Byrsa, 8 [1958-1959], pp. 158-159; cf. already above, p. 15), and my statement is evidenced by Robin's translation of ʾdm and ʿbd as "clients" and "esclave," respectively (cf. RoPH, p. 574 and 578; respectively, and RoHT, I, p. 183). Finally, his last statement is a truism: how could there be an antinomy between being an "inhabitant" and being a "servant"? But, it is also obvious that Robin's affirmation cannot possibly justify his translation of ʿbd as "esclave." For ʾdm, cf. especially above, pp. 33-34.

(4) His note 235 (p. 141) refers to W.W. Müller (Die Wurzeln mediae und tertiae Y/W im Altsüdarabischen. Eine etymologische und lexikographische Studie, Tübingen, 1962, p. 44)and states that "nous croyons que cet auteur a tort de retenir encore la traduction ancienne de 'colon' que plus rien ne justifie." What Müller has is "ḥwr Angesiedelte, Bewohner," and these two renderings are a manifest copy of M. Höfner translations "angesiedelt" (1935) of RÉS 4329/1, and "Bewohner" (1934) of RÉS 4337 B/11-12. As already pointed out by N. Rhodokanakis in Wörter und Sachen, 12 (1929, p. 100: " In den Inschriften bedeutet das Zeitwort ḥwr in der Grundform w o h n e n , s i e d e l n," the translation of ḥwr as inhabitant is basic, but sometimes that individual was a colonist, as in the case of RÉS 4329/1, as Höfner has it. Indeed, the authors of the text, šʿbn/ḏhrbt the tribe Harbat, state that their own town, which they certainly founded, is hgrn/hrbt, since the town bears the name of that tribe. Those people apparently went to found another town, śwm, where they lived at the time of their inscription.

(5) After the statement reproduced above under (3), Robin begins a ploy

of juggling with words and repeatedly contradicting himself in the process. His first number of aerial acrobatics reads as follows (p. 88): "ils peuvent être des hommes libres de la confédération locale (voir CIH 102,4 de ʿAmrān où on lit: ... *bnw Mḏn ʾbkl^{n} ḫwrw hgr^{n} ʿmr^{n} ʾdm bny Mrtd^{m}*) ou des esclaves royaux (Hūr Rūrī 1,1-2 etc)[236]."

The reader remembers that it is in the preceding statement that Robin replaced the word "tribu" of RoPH by "confédération." Thus, there is no difference in meaning between the two nouns in Robin's vocabulary! Moreover, it would be ridiculous to claim, as in Robin's interpretation of CIH 102/4-5, that the Bakilites, ʾbkln, would have been "slaves" of the descendants of Martadum, bny/mrtdm. Finally, as far as Khor Rori 1/1-2 and 2/1-2 (cf. AlAAE, pp. 42-43), it would also be ridiculous to translate ʿbd as "slave." Here, as well as in many other cases, Robin does not realise how impossible his translation is: the two so-called "slaves" would have been sent 775 klm from Šabwat to built a new town and not as ordinary workers. Even the first of the two, ʾAsdum Ṯalʿān, qtdm/hgrn/smrm was in charge of the city Sumhurām for all the stone supply. To send "slaves" so far away would practically hand over to them on a silver platter not only an endless number of escape possibilities but also all means of achieving it.

(6) From now on, the text of RoHT is an addition to that of RoPH. Robin's following affirmation is really a pearl of exceptional value. He writes that:

"On notera aussi [A] que les *ḫwrw*- sont parfois des émigrants, comme l'a reconnu A.F.L. Beeston (CIAS 39.11/06 n°5,2 comm.), mais ce n'est pas [B] toujours le cas: dans CIH 102,4 déjà cité, ou dans CIAS 39. 11/06 n°5,1-3 (*Sʿdʾwm w-ʾbkrb*[...] *Rml^{n} ʾfyš^{n} ḫwrw hgr^{n} Ṣnʿw ʾdm mlk^{n}*), la ville habitée se trouve sous le contrôle de la tribu dont les auteurs du texte se réclament[237]."

Ad [A] - The text involved is YM 350 (cf. CIASA, I, pp. 83-85). Note first that, although Beeston suggests his opinion as "probably" in his commentary, he translates the word as "emigrants" without any question mark. In the text, the proper word is not "emigrants," but "immigrants" because Ṣanʿāʾ is the town to which, according to Beeston, the authors of the text move. But, here again, where is the proof that those persons moved to Ṣanʿāʾ, viz. that they were born somewhere else? Nobody is called emigrant or immigrant if he lives where he was born. There is not a single word in the text suggesting even remotely that those persons were not born in Ṣanʿāʾ. Note also that Beeston restores "dy" at the end of l. 1, which Robin does not accept since he substitutes "[...]." However, the three dots in Robin's transcription mean an indetermined number of letters. In fact, there is space for three letters only, and there is enough of the first two letters to ascertain that they belong to bn; thus ʿbn'[y] the [two] descendants of. Furthermore, Beeston's antiquated, erroneous translation of dhb as "golden" is not even listed in SDDS (p.38); cf. already above, pp. 35-36. Moreover, his translation of ll. 8-12 does not at all reflect the text of his transliteration, as shown by the following reproduction of the text:

"'W
9) ... WZ]ʾN/ʿLMQHT̲HWN/S'D
10) ..... /ʿBDY]HW/S'D'WM/W'BK
11) RB..... M/WMNGT SDQ
12) ..... L'WM" (p. 83).

"And may] ʿLMQHT̲HWN lord of ʿWM continue to grant [10] [and bestow upon His servants] S'D'WM and ʿBKRB [prosperity] and good luck. [By ʿLMQHT̲HWN lord of] ʿWM" (p. 84).

a - Beeston's "And may]- - continue" is erroneously restored "wz]ʾn," which does not exist in such a pericope; it has to be lyz]ʾn, e.g., as in Ja 2118/6-7. This restoration at the beginning of l. 9 gives 19 letters, as in l. 8.

b - Beeston's "T̲HWN" has no counterpart in the original text.

c - In his translation of "WZ]ʾN," Beeston does not even follow his SDDS's (p. 167) oracular renderings; cf. above, p. 39.

d - His "bestow" in l. 10 probably is hmr (cf. SDDS, p. 61), which is not restored in his text and which also follows sʿd in CIH 314+954/24.

e - His "[ʿBDY]" in l. 10 means "two servants", not "servants," as in his translation.

f - His "ʾBKRB" of his translation of ll. 10-11 should read "ʾBK[RB]."

g - The "prosperity" of his translation of l. 11 has no counterpart in his transliteration. It is probably "wfym" (cf. YM 470/5 in CIASA, I, p. 222 although SDDS does not mention such a rendering; it has "safety; success; well-being" (p. 158). My copy of the stone has ⌊ym⌋; thus, [wf]⌊ym⌋.

h - Since Beeston's translation does not show any dots between his "ʾBK[RB]" and his "prosperity," as his translation now stands, l. 11 would be composed of 14 letters only, instead of 19 (ll. 8 and 9) and 20 (l. 10); thus one word precedes [wf]⌊ym⌋; cf. nʿmtm as in nʿmtm/wwfym/wmngt/ṣdqm in Gl 2655/21-22; thus, with nʿmtm l. 11 also has 20 letters.

i - With the restoration of one flower on the right side, Beeston's reading of l. 12 contains 4 letters too much, viz. his "thwn," which is found nowhere in the text, must be omitted.

Thus, ll. 8-11 may be read as follows:

8 - - - /w
9 [lyz]ˌ ʾˌn/ʾlmqhbʿlʾwm/sʿd
10 [whmr/ʿbdy]ˌhˌw/sʿdʾwm/wʾbk
11 [rb/nʿmtm/wwf]ˌymˌ/wmngt/ṣdq
12 [Flow- m/bʾlmqhbʿ]lʾwm Flow-
er er

8 - - - And
9 [that may conti]nue ʾIlumquh, lord of ʾAwwâm, to make happy
10 [and vouchsafe to] His [two worshippers,] Saʿadʾawwâm and ʾAbka-
11 [rib, prosperity and safe]ty and [also] storms perfect.
12 [Flow- By ʾIlumquh, lor]d of ʾAwwâm. Flow-
er er

Add to Pirenne's description of the stone the following details. The upper side shows two almost circular cavities, 0.3 cm apart and 1.5 cm away from the front edge. Diameter: 2 (left cavity), and 2.3 cm (right cavity).

Ad [B] - This is the pearl of Robin's argumentation. It is simply flabbergasting to see the second text mentioned by Robin is e x a c t l y that which he refers to only a single line above to prove the opposite, viz. "ḫwrw- sont parfois des émigrants." Does Robin really think that his reader is blind? What is the reader supposed to think when he reads Robin's wheedling statement in his short untitled preface (p. IX): " Nous remercions les membres du jury pour les suggestions et les critiques qui ont permis d'améliorer cette étude, et tout particulièrement Jacques Ryckmans qui a suivi de très près toute son élaboration."

Of course, nobody knows what "de très près" means in Robin's jargon. It seems obvious that those jury members and especially J. Ryckmans did not even read Robin's expose since it is not here a question of remembering on one page what Robin wrote even on the preceding one; it is just ONE SINGLE LINE apart IN THE VERY SAME PAGE!!!! How then is it possible for the reader to view Robin's statement displayed in the last four lines of an 11-line foreward which occupies the whole p. IX and introduces the publication itself, viz. the phrase reproduced above, otherwise than as a voluntarily untrue ploy aimed at both flattering the persons involved and deceiving the reader? What other proof does the reader need to sternly stigmatize the four jury members and especially J. Ryckmans for an unethical job - provided that they did anything at all except to listen to more or less periodical reports - and also to simply disregard Robin's reasonings and theses since he apparently is unable to build an argument without making a fool of himself in two consecutive lines?

(7) Robin's preceding blunder would doubtlessly be ample reason for stopping right here all study of Robin's "thesis" which was

"préparée sous la direction de Maxime Rodinson, directeur d'études à la IVe section de l'Ecole pratique des Hautes-Etudes (éthiopien et sudarabique)" and

"Le jury était composé de Paul Garelli, président, Maxime Rodinson, rapporteur, André Caquot et Jacques Ryckmans" (p. IX).).

However, one has to complete the job he has undertaken, no matter how repulsive it may be. Robin's next statement is as follows (p. 88):

"Malgré la racine guèze ḪWR (métathèse de la racine arabe RWḪ?) qui signifie "aller", il ne semble pas que le sudarabique ḫwrw- contienne l'idée de déplacement, d'émigration. On traduira donc ce mot par "habitants"."

The main phrase repeats what he just said before, viz. his rejection of his own initial statement where he agreed with Beeston's interpretation of YM 350/2 (cf. above, p. 72, no. (6)). Furthermore, what is the fuss about the Geʿez verb, which means "ire, incedere, vadere, profiscisci" (cf. A. Dillmann, Lexicon linguae aethiopicae, New York, p. 114A, and W. Leslau, Comparative Dictionary of Geʿez (Classical Ethiopic), Wiesbaden, 1987, p. 249B)? Normally an individual does "walk" where he lives and "move off, set forward" from the place where he resides.

(8) Robin's final statement is as follows:

"En opposition avec ḫwrw-, il semble que les ʾbʿl sont de véritables "citoyens" de la cité en tant que "propriétaires" (ʾbʿl dans son sens concret) de la cité et de son territoire" (p. IX).

There is no basic opposition between the realities behind the nouns, viz. the ʾbʿl are no more "véritables citoyens" than the ḫwrw. The two words practically mean the same, but the latter puts the accent on the fact that the persons "live" in the houses and on estates which they own, and the former that they "own" the houses and estates where they live. That the ʾbʿl/hgrn are "propriétaires' - - - de la cité et de son territoire," as Robin puts it, is a simplistic exaggeration, such as šʿbn/sbʾ/ʾbʿl/hgrn/mrb (e.g., RÉS 3910/2) cannot be taken literally as if meaning that the tribe did own the totality of the city Mārib because there were other peoples not belonging to that tribe that owned houses in the city; e.g., the authors of Ja 562, 616, etc.

Ll. 5-6: kwḫ, cf. Ar kâḥa (i) "penetravit(gladius)" (cf. FrLAL,IV, p. 73B). The verb designates the engraving act as digging into the material.

L. 6: mʿhn, cf. Ar ʿahana "to remain, stay" and ʿâhin "lasting, enduring" (cf. LaAEL, p. 2185A-B). The noun puts the emphasis on the lasting condition of an engraving made in the rock.

Ll. 7-8: the number is translated by the coeditors as "cent quarante six"[sic!]

(p. 84).

Ll. 8-9: ʿmrtʿ instead of "ʿmrt" of the coeditors (pp. 84, 85 and 87). The great importance of the text is not the number of the date itself, but that it describes the social position, qwln the ruler, of the person whose identity card became the appellation of a new comput. Furthermore, the clan affiliation given as dšm is known so far in Qat only, viz. in a rock inscription from the country of Mukerās. This feature raises the question of the Qat origin of bkrn.

The coeditors end their interlude as follows (p. 85):

a – They refer to the comput of "CIH 448 + Garb., Hakir 1." This siglum is as erroneous as G. Garbini's: "CIH 448 (Hakir 1)" (cf. AION, 31 [1971], p.303). Since the central section of the original stone is still missing, the siglum is CIH 448+.+Hakir 1 or, more conveniently and more simply (contrary to Robin's relish for cumbersomely long sigla; cf. also JaMAR, XIV, pp. 269-270), Ga 51.

b – The coeditors continue saying that

"Le site de Hakir a livré un autre texte daté, Gr 40 = Garb., Hakir 2, qui porte le chiffre de 167, sans indication de l'ère employée. Comme 167 est assez proche de 146 (Nāǧī 1), il pourrait s'agir de la même ère locale."

This is the type of superficial arguments Robin's thesis is loaded with. It is erroneous on two counts.

1 – The end of Ga 52 is m³t][... The coeditors gratuitously restore the word as m³t][m = [one] hundred. The restoration of the word as m³t][n] or m³t][yn] = [two] hundreds is as probable as the other. Therefore, any argument based on the date "167" considered as certain (cf. also p. 87) is erroneous.

2 – The number of a date by itself has nothing to do with its identification. But, the urge of systematisation is so strong that the coeditors, although fully aware of the difference between Ga 52 with NO name at the end of the comput and their "Nāǧī 1" with ONE, voluntarily suggest that the comput appellation could be the same in Ga 52. This is all the more inconsiderate that they did not even have at their disposal the palaeography of their "Nāǧī 1." One characteristic of Ja 3199 is the constant dextrograde ductus of m (cf. above, p. 69). The dextrograde ductus of m also exists in Ga 52/5, but it is immediately clear in the photo of Gr 40 that the left concave line of the letter is the result of a distraction of the engraver. Had he wished to engrave a dextrograde m, he would have engraved the left line of the letter as a straight line; yet, he also engraved it as a concave line. Another distraction of the engraver is seen in the following t of m³t][, where the center of the right half is rounded instead of being sharply angular as in all other t's of the text. Finally, and also very significative of the coeditors' argumenting method, their opinion on Gr 40 and their "Nāǧī 1," although presented hypothetically (cf. their "pourrait"), becomes a certainty on p. 87!

From a palaeographic viewpoint, Ga 52 is much later than Ja 3199 as indicated by the archaic r of the second and the strongly undulated form, as in a crossbow, of the first.

A final remark concerns Ja 2356a. The coeditors speak of

"VL 29 a du Wādī Širǧān (daté 345) et MAFRAY-Ḥaṣī 5 (daté 365)[20], inscriptions qui mentionnent le même qayl de Mḍḥy^m avec trois ou quatre de ses fils" (p. 85).

The second text mentioned still remains unpublished, but in Ja 2356a (cf. JaMAR, II, pp. 89-90 and tracing in pl. 7), the name mḍḥym does NOT exist; the date is wrḫhw/dʿddn dlḫmst/w³rbʿy wtlt/m³tm ḫrftm (ll. 9-12). The mr³ lord of the two authors of the text, who is mentioned in l. 4, and the names of his three sons (ll. 5-6) are said to be bny/hṣbḥ/wyʿlf descendants of Haṣbaḥ and of Yaʿlif.

The preceding presentation of the case of Ja 3199 reflects a situation well in the line of Robin's reasoning and argumenting method, as already denounced in JaMAR, X, pp. 34-70, and denounced again at the occasion of new examples, including the pearl of his thesis in l.c., XIV, pp. 269-303.

10 – ³rbʿw in SA titulatures.

The following example is given here to show the dramatic schematization of ordinary concepts under the impulse of preconceived ideas.

Ir 40 begins with a long titulature which reads as follows:

šrḥʿtt/y³mn/bn/drnḥ/ ³bʿl/bytn/³ḥrm/
³qwl/šʿbn/dmr
³rbʿw/qšmm

Robin translates the preceding pericope as follows (cf. Ṣayhadica, pp. 114-115, and commentary on pp. 115-118): "S³rḥ°tt Y'mn ibn Drnḥ, seigneurs du palais ʿḥrm, qayls de la tribu Dmr, fractions de Qs²m^m"

a – Robin's translation of ³bʿl/bytn as "seigneurs du palais" is pompous (cf. already above, p. 66) and remains unjustified in his commentary (p. 115), except by his determination of making the drnḥ some kind of princely group.

But, here again, "seigneurs" and "propriétaires" are synonymous in Robin's vocabulary (p. 132) in the same manner that "citoyens" and "propriétaires" are synonymous (cf. RoPH, pp. 118-119 = RoHT, I, pp. 88-89; cf. above, p. 70). The expression simply means masters [= owners] of the house ʾAḥram. It is quite natural that a family, drnḥ, would own its own house, ʾḥrm. But, such a reality is too simple and too humdrum for Robin. He also refers to "CIH 537 + RES 4919/4" in connection with ʾḥrm. The siglum is erroneous; it is RÉS 4919 + CIH 537.

b - ʾqwl. - It is immediately obvious that, even in his transliteration of SA words, Robin follows his own idea: he does not explain why he transliterates ʾqWl as "qaYls." Furthermore, Robin, who always gives the impression of translating everything as accurately as possible, remains silent on the meaning of the words qwl: he limits himself to transliterating it as "qayls" (pp. 114, etc.)

c - The two preceding words, bʿl and qwl, are discussed in Robin's thesis (cf. RoHT, I, pp. 88-89 and 79-87, respectively; this last section of RoHT, I, corresponds to RoPH, pp. 105-107). It is difficult to follows the author's messy reasonings in which schematization and speculation are constantly used. In the case of qwl, not a single rendering is suggested. The author, however, had said this in 1981: "on pourrait rendre [the word qwl] par 'prince' ou 'baron'" (cf. CRAIBL, 1981, p. 323), a translation which, of course, fits ... a princely group and a "palais" ... provided that Robin proves - but he never does - that the group is ... princely. When reading Robin's prose and his thesis, I have had many times the impression that Robin was living in the Middle Ages with his vocabulary of "prince," "baron," "palais," etc.

Incidentally, in the same paper of CRAIBL (also p. 323), Robin renders a pericope of no.2 of al-Maʿsâl as follows:

> "le roi Karib'il Ayfaʿ, seigneur de l'auteur, et l'armée himyarite restent sur place le temps qu'il leur plaît puis s'en retournent à la cité de Hakir avec leurs trophées, hommes et chevaux vivants et tués."

This is doubly incredible: not only that an army would have taken back home a booty described as "men and horses, alive and KILLED," but above all, that they would have achieved such an unheard-of novelty a f t e r remaining on the battleground "le temps qu'il leur plaît"!!! It should also be noted that in RoHT Robin, after dealing with qwl, devotes 6 pages to what he calls "les autres catégories sociales" (pp. 87-93), of which the last section entitled "les esclaves" (pp. 89-90) deals with the two words rbb and ʿbd (cf. also pp. 184 and 183). The SA word for slave is qny (cf. above, pp. 6-7), not ʿbd, with its plural ʾdm servant(s), worshipper(s), and rbb means owner, regardless of what is owned; e.g., in CIH 308/24-25, it would be ridiculous that the vanquished would have sent as hostages to the kings two "slaves." That would have been a downright insult; the two persons involved had obviously enough to have a very respectful social standing. The penult paragraph of that section (p. 90) is a note which begins as follows: "Il n'y a pas lieu de reprendre ici les développements de A.G. Lundin sur l'esclavage en Arabie du Sud;" and Robin mentions in particular Lundin's etymology of hgn as "Ar hajîn 'son of a freeman and a slave mother'." Robin states that hgn is "un terme de sens peu sûr." Yet, it is that very meaning followed by a question mark, which is reproduced on his p. 183. Of course, Robin prefers to ignore that I have already rejected Lundin's social synthesis in 1965 in Research on Sabaean Rock Inscriptions from Southwestern Saudi Arabia, Washington, pp. 14-19; in particular, I related hgn not to Ar hajîn, but to Ar hijân "born of a noble and illustrious family" (p. 17). But, Robin could not possibly have read my book because it is not even listed in his bibliography; cf. II, p. 160!!!

d - ʾrbʿw/qšmm. - Robin writes this (pp. 115-116):

> "Dans Ja 650/2, l'éditeur traduit ʾrbʿw par "fourths", sans commenter son interprétation."

His statement may be correct, it all depends on his own vocabulary. Unfortunately it still remains unpublished. Therefore, it is to be assumed that the verb "commenter" is given its ordinary meaning. If such is the case, it is impossible to understand his remark since the commentary on ʾrbʿw of Ja 650/2 reads as follows in JaSIMB, p. 154B: "L. 2: *hwtn*, cf. the Thamudic personal name *hwt* (cf. *vdBrTTPS*, e.g., p. 97: Ph 167 ao). - ʾrbʿw (also in lines 24-25), plural of ʾrbʿ (cf. *HbASG*, p. 104, §87; cf. also commentary on Ja 615/18-19); considered as noun."

Is it not dishonest to affirm the inexistence of a commentary published in the right place? Furthermore, it is irrational to expect a commentary published in 1962 to specifically answer his invalid concoctions published 25 years later. Moreover, Robin does nothing but repeat Beeston's verdict: "without explaining (what is very far from apparent) what the plural here could imply" (cf. Le Muséon, 88 [1975], p. 189). If such is the case, both Beeston and Robin appear to be so intellectually limited or impaired as to not understand that which is obvious, viz. "the fourths" are the individuals composing the "fourth," in the same way as sbʾ Sabaʾ and ʾsbʾn the Sabaeans, hgr town and townsmen, hlf country and countrymen, etc.

Robin, then, states that in his thesis

"nous avons traduit *rbᶜ* par "quart", en nous fondant sur les trois "tiers" de *Sᵊmᵊy* et, de manière implicite, sur les tribus contemporaines dont les fractions sont appelées *rub* ..., hums..., suds..., tusᶜ... etc" (p.117).

The initial statement is correct; cf. his statement "'quart'" (rbᶜ)" (cf. RoPH, p. 59, which is repeated verbatim in RoHT, I, p. 45).But, in both publications, the word "quart" disappears and is invariably replaced in the text without any explanation by the word "fraction"" (pp. 59, 60, etc., and 45, 46, etc., respectively).

The second statement, "et, de manière implicite, ...," cannot be proven correct because Robin's argumentation on rbᶜ is based solely on SA material; therefore, that second statement can only be understood as a postmortem justification. Furthermore, modern administrative divisions are irrelevant to SA. But, if Robin finds them relevant and, in fact he does, then, they disprove both his and Beeston's opinions because neither Beeston nor Robin can deny the obvious: rubᶜ, plural ʾarbaᶜ one fourth, hums, plural ʾahmas one fifth, etc., illustrate the SA divisions, and SA ʾrbᶜw corresponds to Ar ʾarbaᶜ.

Robin refers to Beeston as saying that "

"cette traduction était trop dépendante de l'étymologie comme l'a remarqué A.F.L. Beeston" (pp. 117-118).

But, Beeston does not say that at all and all the more so since, in order to solve his own problem, Beeston refers to another word of the very same root rbᶜ, viz. "Arabic rabᶜ 'dwelling-place' rather than with rubᶜ 'one-fourth'" and, thus, "ʾrbᶜw becomes 'townfolk'" (cf. Le Muséon, 88 [1975], pp. 189 and 190, respectively). As a matter of fact, Beeston missed the mark: the best reference he should have retained to prove his point is rubūᶜ, pl. rabᶜ "the people of a place of alighting, the people of a house or tent, a company of men or people" (cf. LaAEL, p. 1017A). Even such an etymology does not justify SDDS's "townfolk(?)" (p. 113) because the choice of the Ar word is unsupported by the texts which speak of membership of a tribe, not of inhabitants or transients.

Robin ends his argumentation saying that

"Il serait préférable de supposer que le sudarabique est passé de la notion de "quart" à celle de "fraction", évolution comparable à celle du français "quartier" qui n'implique plus une division par quatre."

Robin is naive if he thinks that his example is limited to French and, especially, that it proves anything beside breaking into an open door. The question is to know whether the preferability of his thesis is justified or not. In fact, that preferability is nothing but a preconceived idea which the texts do not justify. What Robin does not include in his argumentation is the reference to his own thesis which acknowledges that

"Comme trois seulement sont attestées, il n'est pas impossible qu'une quatrième reste à découvrir" (cf. RoHT, I, p. 45, repeated verbatim from RoPH, p. 59). Therefore, Robin contradicts himself since he does not prove the impossibility of what he admits as possible in his thesis. But, instead of informing his reader of his own acknowledgment, he sweeps it under the rug so that the reader might not be able to turn against him his own statement. Even if the last "fourth" is never found epigraphically, the situation would not change and both Beeston's and Robin's opinions would still not be confirmed because it may well remain impossible to know the exact origin of the "fourth." It could well have been a structural plan conceived in the early stage of the SA society limiting the increase of a tribe to a fourfold group.

The last 36 pages (pp. 44-81) have once more pointed out so many extravaganzas in Robin's reasonings, argumentations and theses that the reader can hardly escape from remembering the French slogan, "thèses, hypothèses, synthèses ... et foutaises." Those pages amply justify my initial judgment made after a quick glance at Robin's thesis and published in JaMAR, XIV, p. 303, as follows: "The preceding presentation of C. Robin's work, including a part of the epigraphical section of his thesis, contains the basic reason of my unwillingness to review the theoretical section of that thesis, which is based on the kind of aberrations and distortions of the epigraphical material such as shown in the preceding pages; that section is not worth discussing."

B - F. Bron, the Qat expression rēv/ʿm priest of ʿAmm, and Ja 3200.

F. Bron publishes in Sayhadica (pp. 21-27 and photo in pl. 2) a Qat inscription "provenant de Timnaᶜ."

It seems certain that, although the stone was allegedly found in Timnaᶜ, the Beihan museum, which is also located in Timnaᶜ, was apparently unable to verify that information or to find out the exact location where the text came from. The finding of the stone most probably occurred at the occasion of clandestine excavations which have been reported many years ago in the northern vicinity of the South Gate where several buildings were uncovered. At any rate, the case of this inscription illustrates that of RÉS 3965 (cf. above, pp. 17-18) which has been discovered almost 70 years ago and has been in the Aden Museum for

about fifty years. Furthermore, the siglum given by the editor, "Musée de Bayḥân," cannot be official, and the reader wonders why the official siglum in Ar or in English (cf. NAM = National Aden Museum) is not even mentioned between parentheses or in a footnote. Therefore, I will retain for texts of that collection the siglum M[aṯḥaf] B[eiḥân].

Here follows the photo published in the edition:

Bron reads the inscription as follows:

" 1 *Ś₂ḥa/ʾlʾl/yhnᶜm/bn/ʾydᶜ·b/mkrb/mʿṯbn/bnỷ/ʿm₁ḥd̲ṯ/t*

2 *ḫmy/gʿn/S₃dw/tᶜm/dḏwnm/bᶜl/S₃dw/ywm/ʿs₁m/ᶜmm/s₂ṯṯn*

1 *S₂ḥa ʾlʾl yhnᶜm*, fils de *ʾydᶜ·b*, mukarrib de Qatabân, a bâti et inauguré

2 l'enceinte du jardin de *S₃dw* pour ᶜAmm de Dwan, maître de *S₃dw*, alors qu'il était prêtre-éponyme pour la troisième année."

The editor's superficial work is basically vitiated by several facts of which the most important are his failure of utilizing the right parallels and his erroneous understanding of the eponym comput.

L. 1: šhr/hll/yhnᶜm/bn/ydᶜ·b/mkrb/qtbn is the first example of that titulature, a fact unnoted by the editor and which justifies the restoration of yhnᶜm in RÉS 3950/1 (cf. photo in CIASA, II, p. 137, with Pirenne's extravagant translation in p. 136) and also in TC 1176+2160 (cf. JaNPIO, pp. 46-47), a text which is not mentioned by Bron. The editor identifies "sans doute" this mkrb with his namesake king of RÉS 3691 and 3693. This identification remains to be proven, and no proof is suggested by the editor. The three texts, RÉS 3691, 3692 (not mentioned by Bron) and 3693 are by the same king because they are engraved above each other on the same large slab of the South Gate of Timnaᶜ (cf. JAMAR, III, pp. 61-62): RÉS 3691 is below; RÉS 3692 at the top and RÉS 3693 in between. Incidentally, it is incomprehensible that J. Pirenne, who visited that South Gate of Timnaᶜ, would still reprint N. Rhodokanakis' sketch (cf. CIASA, I, p. 115) without even noting its wrong repartition of the texts. His full identity card, šhr/hll/yhnᶜm/bn/ydᶜ·b, being in RÉS 3691, the king did not deem it necessary to repeat it in its entirety in his next two inscriptions. To the right of RÉS 3691 and below the right section of RÉS 3693 is RÉS 3854 by the preceding king šhr/hll/bn/drʾkrb.

Bron does not list RÉS 3692, as noted above, because of a reason given in his footnote 5 in p. 24 and which reads as follows:

" 5 D'après Beeston, l'auteur de RÉS 3692 serait non pas *S₂ḥa ʾlʾl*, mais *S₂ḥa Ygl*, cf. Qahtan, fasc. 2 (Londres 1971), p. 16."

Indeed, Beeston writes this (cf. The Labakh Texts, London, 1971, p. 16): "R.3692 - apart from a different date - repeats almost verbatim the provisions of the earlier part of R. 3689 down to the dating."

On RÉS 3691, RÉS had already noted: "pour le commentaire, cf. R.É.S., 3689, dont le contenu est à peu près identique" (V, p. 273), and on RÉS 3692, "inscription à peu près identique à R.É.S., 3691. Voir aussi R.É.S., 3689" (cf. l.c., p. 275). Therefore, without going into any detail, RÉS considers RÉS 3692 closer to RÉS 3691 than to RÉS 3689. The differences pointed out by Beeston to solve the problem of authorship of RÉS 3692 actually are irrelevant to the problem because they can be understood either way. The important difference in the two texts, which Beeston does not refer to, proves that RÉS 3689 stands alone, while RÉS 3691 and 3692 go hand in hand as was already indicated by the location of the text on the large slab of the South Gate. The following expressions are noted:

	...but...
t³ntsm of RÉS 3689/4	t³ntsm/w³wldsm in RÉS 3691/4 and 3692/3;
t³ntsm of RÉS 3689/6	t³ntsm/wbnysm/wbnytsm in RÉS 3691/5 = t³ntsm/wbnsm in RÉS 3692/4;
bᶜly of RÉS 3689/6	bᶜlw in RÉS 3691/5 and 3692/4;
mhrtn of RÉS 3689/6	mhrtn/wqrytn in RÉS 3691/5-6 and 3692/4;
mhrtn/w³strn of RÉS 3689/7	mhrtn/wqrytn/w³strs in RÉS 3691/6-7 and 3692/5;
bd/ġyln of RÉS 3689/4	bdġyln in RÉS 3691/8 and 3692/6.

Therefore, both RÉS 3691 and 3692 have the same king, šhr/hll, for author.

Speaking of that mrrb, Bron also states (p. 24) that

> "Il faut le situer, semble-t-il, au début du deuxième siècle avant notre ère.(6)"

This Pirenne's opinion is nothing more than a guessing based on a fallacious palaeographical system which does not deserve any consideration.

šhdt: the history of the recent translations of the verb, which were initiated by A.F.L. Beeston (cf. Le Muséon, 85 [1972], p. 541), is very interesting. Beeston argues that šhdt "could simply imply that the object was 'newly made', i.e. brought into existence for the first time, as well as that it was 'made new' or 'renewed'. We must not infer that all the constructions to which these terms are applied in RSA were necessarily re-constructions of a previously existing building." The three following points are clear from Beeston's prose, viz.

a – šhdt may have two different meanings;

b – all the texts do not commemorate restorations or remodelings of existing buildings; and

c – Beeston fails to give the rule or criterium which may indicate when either meaning must be applied.

Instead of expliciting the criterium which, according to him, makes the choice of a meaning possible in any given text, he embarks – as he usually does – on an irrelevant comparison: he refers to "a precisely similar point in pre-Islamic poetry." Who, in his right mind, would even dare to compare a house with a tattoo?

J. Ryckmans' answer to Beeston is phrased as follows: "one might question (at least in some instances) the rightness of the new interpretation proposed by Beeston... 'has newly done'" (cf. PSAS, 4 [1974], p. 138). Ryckmans' starting point is totally irrelevant to the question, viz. the lack of documents attesting the transfer of ownership. But it is precisely this point which he intended to inject into his translation of hbdt. Indeed, he concludes his silly argumentation as follows: "the erection of a building or the opening up of a fallow land are the most primitive and fundamental ways of acquiring real estate. The insistent use of the verb hbdt seems to betray the intent of the new owner to make the acquired foreign property truly his own." What a stupid argument! Such a ridiculous description exists only in Ryckmans' imaginative dream. The texts do not speak of people establishing themselves in a no-man's-land; they talk of building a house in their community. The simple fact of building a house or of tilling a previously uncultivated land is not a way of "acquiring real estate" except in

a no-man's-land. The verb hbdt implies a material fact, not a juridical act, so Ryckmans' opinion is all the more out of place that Sa has verbs clearly expliciting the juridical aspect of change of ownership, viz. ʿs², ś²m and qny. Furthermore, here again, Ryckmans gives another example of his idiosyncratic argumenting method: he does not explain to his reader why, in his opinion, Beeston's suggested translation might not be applicable "in some instances;" he even fails to give a single example of those "instances;" finally, he also fails to give his translation of hbdt. How easy it is to fill up pages after pages with empty and silly words!

Finally, the whole picture drastically changes with SDDS, where hbdt is given two translations: "build, found, establish" (with reference to the above-listed paper of Beeston) and "initiate, inaugurate" (without any reference) (p. 65). For my lengthy review of SDDS, cf. JaMAR, XIV, pp. 62-269. The meaning of "to renew, renovate," although still clearly maintained by Beeston, vanished in the thin air. Furthermore, both bny (p. 29) and hbdt would mean the same, "to build;" thus, a pericope, such as bnyy/whbdt (e.g., Ja 554) would involve a tautology, "have built and built." It is most probably to be in a position of avoiding such a tautology that SDDS did invent a new erroneous translation, viz. "initiate, inaugurate," two verbs which are not even synonymous ... and it is the second translation which Bron endorses in his paper ... without even giving the slightest etymological justification.

Ll. 1-2: the word is read by Bron as "thny l'enceinte."

1 – Because of an unexplained illogical attitude, he discards the fact which he himself acknowledges, viz. that the root hny is used "dans des contextes relatifs à l'irrigation." This is illogical because anything relating to irrigation would be relevant in a text dealing with a garden, and the reader would certainly like to know the reason, which must have been irremediably compelling, why he eliminates all those Sa texts dealing with irrigation. Instead, he turns to something else which he must have thought to be a more appropriate and more acceptable solution. Indeed, he states that "thny est un substantif dérivé de la racine ḥny, 'protéger', utilisée en sudarabique dans des contextes relatifs à l'irrigation, mais qui donne en phénicien et en hébreu le nom de l'"enceinte de ville"" (p. 23).

The preceding etymology is disputable because the root ḥwm is also attested in Daṯīnois (cf. LaGD, I, p. 528 and especially p. 530); the same idea of "circling" is also found in the Middle-East Ar: "ḥwm ... tourner inutilement pour trouver qqe ou par indécision" (cf. A. Barthélemy, Dictionnaire arabe-français. Dialectes de Syrie: Alep, Damas, Liban, Jérusalem, Paris, I [1935], p. 186. In Phoenician, J. Friedrich and W. Röllig (cf. Phönizisch-punische Grammatik, Rome, 1970, p. 98, par 204a) make it certain that the -yt ending of ḥmyt "(Mit Mauern ausgestattete) Befestigungen" is the feminine ending; cf. also R.S. Tombach, A Comparative Semitic Lexicon of the Phoenician and Punic Language, Missoula, MT, 1977, p. 108: "ḥmyt fortresses;" St. Segert, A Grammar of Phoenician and Punic, Munich, 1976, p. 289: "ḥmt: pl. ḥmyt wall;" and J.C.L. Gibson, Phoenician Inscriptions, Oxford, 1982, p. 169; "ḥmyt plur. 'walls'." Yet, it is against such a consensus of "Befestigungen," "fortresses" or "walls" that Bron translates ḥmy as "enceinte."

2 – It is also immediately obvious that the upper left corner of the stone is broken off (cf. above, p. 82 for the photo of the stone) and that, therefore, one letter is missing immediately above the last m of l. 2; thus, t[.]ḥmy. It would be abnormal to assume that that space would have remained empty of any letter, but it is unexcusable of the editor not even to mention the broken corner as if it were nothing. The word that immediately comes to mind is mfḥm in Min RÉS 3327/6, and t[f]ḥm may be the place where the burning (cf. the abstract initial t-) of charcoal (cf. Ar faḥm and faḥmat a piece of charcoal; cf. H. Derenbourg in The Babylonian and Oriental Record, 1 [1886-1887], p. 202) is done. Charcoal is always used in incense burners. But, the two verbs of l. 1 make certain that t[f]ḥm is a building, not an ordinary incense burner. Incidentally, note that Derenbourg worked from Glaser's facsimile which does not include the word divider at the end of l.6. That word divider was restored by D.H. Müller (cf. Kritische Beiträge zur südarabischen Epigraphik, in WZKM, 2 [1888], p. 209, but was lost again by both RÉS (V [1933], p. 104; cf. already in JaMAR, II, p. 62) and A. Capuzzi (cf. Iscrizioni minee, Naples, 1974, p. 88). It may be inferred from the first part of l. 2 that śdw was a garden with the temple dwnm to which the king added two incense sanctuaries. It is also known from the position of Ja 2436,2437, RÉS 3691, 3694 and 3854, where the expression ḫlfn/dśdw is found, that the South Gate of Timnaʿ was that ḫlf and, thus, was in some way facing the field of śdw. This field or garden must have been rather extensive because, beside the property of the deity, there were also estates belonging to some individuals (cf. RÉS 3854 /2). It is not possible to deduce from the present text that the expression [illegible] śdw of l. 2

"semble indiquer que la porte de la ville lui était dédiée, ou peut-être qu'il possédait un temple dans son voisinage" (p. 23).

The expression simply means that the South Gate was the normal exit for the inhabitants of Timnaʿ to take if they wanted to go the śdw gardens. This does not even prove that śdw was contiguous to the South Gate. The exact location and extent of śdw remain unknown; therefore, it is not possible to know where the temple dwnm and/or the two incense sanctuaries were located and certainly not that they were in the vicinity of the South Gate.

L. 2: gn: Bron refers to Ja "2843," which must be corrected to 2834. – ywm/rśw/ʿmm/śltm is the most important section of the text to which Bron devotes the best part of his paper and the very best of his attentive care.

a – He immediately lays down the result of his careful investigation by declaring that the pericope "oblige à revoir l'interprétation des syntagmes ʌʌ$_{\text{ʒ}}$ʷ ʿmm (RES 311/1, 3880/3, Lu 30-40)[7] et ʌʌ$_{\text{ʒ}}$ʷ ʿmm ṯaim (RES 3540/3-4, 3880/3-4)" (p. 24).

To the references to rśw/ʿmm, add Ja 2436/1. – Bron's reference to "Lu 30-40" is incorrect because Lu 35 is composed of the personal name ʾwsʿm only, and Lu 33 and 34 are very fragmentary (cf. A.G. Lundin, Spisok zhyeshov ʿAmma, in PS, 27 [90] 1981, pp. 28-29; and no. 33 is not readable in the photo of p. 27). Furthermore, in Lundin's transliteration and translation of Lu 40/1 (p. 33), which are repeated in Raydān, 2 (1979), p. 110, note 7, "ddrḥn" must be corrected to ddrʾn according to the photo of p. 33. Moreover, neither Lundin nor Bron note that the whole text of Lu 36 is found in Ja 2436/1; and Pirenne does not even refer to Lu 36 in CIASA, I, pp. 109-116.

On tntm of the Qat expression, Bron states (p. 24) that

"c'est en réalité l'adjectif ordinal "deuxième", attesté en sabéen dans l'expression dm ṯaim (Ja 576/11), "une deuxième fois"."

This expression was already analyzed in JaSIMB (p. 75A) as "a second time." The best parallel of the whole Qat expression is not Ja 576/11, which deals with military activities. The following schema shows which texts are the best parallels.

Qat	RÉS 3880/3-4:	ywm	rʾsw	ʿmm	tntm	when he was the priest of ʿAmm a second time
	MB 3/2:	ywm	rʾsw	ʿmm	šlštm	when he was the priest of ʿAmm a third time
Sab	Ja 2848 ad/3:	ywm	rʾsw	ʿttr	šlt/rʾswtm	when he was the priest of ʿAttar for a third priestly ministry
	Ja 2848 an/3-4:	ywm	rʾsw	ʿttr	tny/ʾd	when he was the priest of ʿAtar the second time

SDDS (p. 2) refers ʾd to "C 366, Cl 1677." But, Cl 1677 is a part of CIH 366 d, one of the quadruplets of the text, as shown in JaCMYE, p. 70. - The importance of the last two Sab parallels is decisive in the case of the Qat texts because they show that ʿmm is a divine name, not a noun, which is Eron's thesis. Of course, the reader cannot but raise the question as to why Eron does not refer once to the Sab parallels. I cannot but consider Eron's silence as at the very least suspicious. Furthermore, in the Ja 2848 collection, rʾsw is always followed by the name of ʿttr (e.g., rʾsw/ʿttr in Ja 2848 l/2) and the expression brʾswthw is always found in a context mentioning ʿttr (e.g., Ja 2848 w/2-3). It is with such contexts in mind that one has to interpret Ja 2848 n/2: ywm/rʾsw/tnytm. Since not a single god beside ʿttr is ever mentioned in the whole collection, the divine name ʿttr is understood in that pericope.

c - Contrary to the above-listed parallels, Eron claims that

> "44ₚ" ʿmm ḫḍn se traduira donc par "prêtre pour la deuxième année"; quant à 44ₚ' ʿmm, "prêtre pour (une) année", c'est, semble-t-il, une manière d'exprimer la dignité d'éponyme" (p. 25).

(1) Eron's translation is erroneous even in his own theory because his wording would demand ʿmH/tntH; but the text has ʿmM/tntM. Furthermore, Eron's expression suggests that the second year immediately follows the first. Such a situation is absolutely gratuitous and cannot at all be proven.

(2) Eron's last phrase is an extravagant dramatization imagined to give some meaning to an otherwise incomprehensible tautology. Since he gives ʿm the meaning of "year," it is obvious that the priestly ministry was for one year. Therefore, what is the justification for expliciting it twice in that rʾsw/ʿmm which he should translate as follows: "when he had the one-year priestly ministry one year." Furthermore, it is regrettable that Eron does not explain to his reader where he found a clue supporting his "manière d'exprimer la dignité d'éponyme." Cf. below f.

d - To substantiate his theory on ʿm, Eron does not fail to rehash J. Pirenne's opinion on "l'invraisemblance que le nom du dieu soit écrit ici avec la mimation, ce qui n'est jamais le cas en dehors de ces deux formules![9]" (p. 25). Neither Pirenne nor Eron mentions the epigraphical fact that wd is the normal name of the Min lunar god, as ʿm is in Qat, but that it is also found as wdm, as ʿmm is in Qat. Therefore, the so-called "invraisemblance" is a dramatic exaggeration, and an erroneous one, of the SA documentation. That wdm is rather frequently used and ʿmm is restricted also is an epigraphical fact. Cf. especially the divine name ʿttrm (instead of ʿttr) of Ja 2860 d in JaCMYE (1976), pp. 104, 218A and pl. 16), which both Pirenne and Eron sweep under the rug!!!

e - Eron then refers to Min "RÉS 2948, dont une photographie de Ch. Robin permet maintenant une lecture assurée" (p. 25).

But, the reading of ḫwn/ʾsd/ʿmm of A/2 was already assured by H.Y. Nami in Majallat kulliyat al-ādāb, 19 (1957), p. 112; and that pericope "reste énigmatique," declares Eron (p. 25). But, the pericope of B/2, wlytb/ʿms/rʾswn/ʿmm

> "ne peut guère se traduire autrement que par "que le prêtre réside avec elle (la tribu?) durant une année"" (p. 25).

The text does not seem much less fragmentary than RÉS's because Eron had to guess at what is the noun referred to by his -s of ʿms. His translation of the pericope is unacceptable because (1) he translates ytb/ʿms in the same way he renders ytbn/bʿm of RÉS 4782/4: "soit dévolue à la tribu" (cf. below), but the preposition b- of the second expression does not exist in the first, and (2) the text should have rʾswn/ʿms instead of the reverse. As it stands, the pericope may be translated as and that may ʿAmis be established as the priest for one year.

f - In his last paragraph (pp. 26-27), Eron confuses the two pericopes ywm/rʾsw/- and hrf/- by giving the first the value of "éponymie" because of the second ... as if the two expressions had anything in common. That an expression, such as hrf/yqhmlk/dḏr²n/qdmn (Ja 2560/14), indicates an eponymate comput is self-evident. The other expression, however, has nothing whatsoever to do with any comput; it is an example of a simple, personal, very common mnemotic aid used by individuals to date an event on the basis of their own personal past, just as people nowadays still say, e.g., the first year of high school, the second year of the army, the third year of university, etc. It is, in my opinion, ridiculous to see some kind of eponymate in such expressions.

f - Now the reader will be able to fully understand Bron's translation of RÉS 4782 which reads as follows: 1 wl/yhbtn/l²lhn
2 fhdm/wmqdmn/bdltn
3 wršyn/lytbn/bᶜm/šᶜbn

Bron's translation (p. 26) is given in two sections which are arranged here according to the lines of the original.

My 1948 translation repeated in JaMAR, XIV, p. 82:		Bron's translation:
"Et qu'il veuille offrir au dieu	1	"Que l'on présente au dieu
une cuisse et deux parties antérieures comme don	2	une cuisse et deux épaules à la porte (du temple)
et cadeau, afin qu'il puisse jouir d'un heureux séjour dans la tribu."	3	que l'éponymie soit dévolue à la tribu"

(1) It is not honest of Bron to still refer to Beeston's theory on "la chasse rituelle" without even alluding to my constant rebuttal of that erroneous theory from 1956 to my last discussion in JaMAR, XIV, pp. 88-90, which starts with "Beeston's spectacular about-face" (pp. 80-82) and then deals with the present inscription (pp. 82-84).

(2) It is also quite surprising that, in his footnote 10 (p. 26), the mention of Beeston's 1948 paper, about which I wrote my note on "Beeston's spectacular about-face" (cf. above), is immediately followed by "voir aussi ABADY, 1, 1982, pl. 34 a et c." The reader's immediate reaction is to think that that reference also concerns "la chasse rituelle." But, it is not true. Bron refers to two photographs showing the pillars and the text RÉS 4782, respectively. Therefore, the second half of his footnote 10 should have been added to his p. 25, under the number "(9bis)" appended to "les environs de Mârib." It is also surprising that, after referring to the two photos, Bron does not mention "pp. 84-85," where they are commented upon from an archaeological viewpoint.

(3) Bron's translation.

(a) mqdm as "épaules" is a copy of SDDS, p. 103 (following Beeston). This rendering is rejected in JaMAR, XIV, p. 83.

(b) bdltn as "à la porte." Bron is forced to invent "(du temple)" to give a meaning to his phrase. This alone already proves that his interpretation is incorrect. Furthermore, he interprets bdltn as b + dltn contrary to SDDS (p. 26: bdlt: "expiatory offering" after Beeston). Moreover, it is ridiculous to even think that the offering to a deity (cf. also his translation of l. 1) would have to be deposited ... "à la porte" even of a temple. Finally, Bron's assumption that his translation of "dlt" as "porte" is based on both Hebrew and Phoenician dlt (cf. his footnote 12 of p. 26) is disputable. Phoenician dlt means "plaque, tablet" (cf. R.S. Tombac, A Comparative..., p. 73. Although "dl 'Tür' is given by J. Friedrich and W. Röllig, Phönizisch-punische Grammatik, Rome, 1970, p. 116, par 240,12, and "dl door" by St. Segert, A Grammar ..., p. 287, J.C.L. Gibson, Phoenician..., p. 140, makes the following remark: "dlt; prob. lit. 'door', here used for a double or hinged tablet." Similarly, Hebrew delet means "Türflügel, Türblatt" and especially daltôt haš-šaᶜar "die Flügel eines Stadttores" (cf. BuWG, p. 163B). That Hebrew delet is translated as "door" (cf. E.Y. Kutscher, Hebräisches und Aramäisches Lexicon zum Alten Testament, Leiden, 1967, I, p. 215A: "Tür. Türflügel"), is explained by metonymy, viz. a part for the whole, and the original meaning of dlt seems to be "that which is hung." Furthermore, there is no reason to assume that SA would have borrowed dlt from either Hebrew or Phoenician.

(c) ršy: for the his translation of the word as "éponymie," Bron calls upon CoNS/2 which he reedited in Aula Orientalis, 1 (1983), p. 140, and photo on pl. 11B. L. 2 reads as follows:

-/bn/gdnm/ᶜlm/wrḫḫw/wršyḫw/wrḥ/blank/dhrf/- which he translates as "banu Gdnm, document dont le mois (vacat) et l'année sont le mois ... de l'année de."

His commentary is as follows: "wrḫḫw est repris par wrḥ,ršyḫw par hrf." First of all, Bron's translation erroneously transfers his "vacat" after wrḫḫw although it is clear that it is located after wrḥ. Secondly, his commentary does nothing but repeat his translation which contains a manifest tautology. Surely ᶜlm governs wrḫḫw/wršyḫw, and ršy is different from ršw, as it is clear in W.W. Müller (cf. H. von Wissmann's Aufstieg und Niedergang der römischen Welt, Berlin, II, 9, 1, pp. 430-431) whose translation reads as follows: "das Dokument seines Monats und seines Opfers" (p. 430). Furthermore, SDDS's second translation of ršy as "year of office of rs²w" (p. 118), which Bron endorses in the present text, is also unacceptable because of the following reasons.

(a) The equivalence ršw = ršy should be proven, not assumed, because of the constantly attested root ršw.

(b) ršw does not at all imply the idea of "year."

(c) The office of ršw is constantly attested as ršwt; thus, even accepted the interchange of the vowels w/y, the word should be "ršyt," not "ršy."

The pericope of CoNS/2 mayn be translated as "the document of his month and of his gift" which means "his benevolent document of the month." Bron also states that "le nom du mois a été laissé 'en blanc', ce qui est peut-être le cas de Gl A 452/6." This statement is incorrect: on the paper squeeze of the text, the place was not pressed or pounded hard enough, as it happened so many times in the collection of the Glaser squeezes.

Bron does not seem to realize the imbroglio of his translation. Who is behind his "on" of l.1? Bron does not give the slightest hint as to the identity of his "on." Furthermore, cf. above, the remark on the incongruity of a gift to a deity to be left "à la porte." Which tribe is meant by "la tribu"? If "la porte" refers to the nearby temple, so should "la tribu;" then, who is "la tribu" of the temple? What are the two offerings for? Is it not ridiculous that the offering of "a thigh and two shoulders," only a small part of any animal, be a prerequisite to a change of personnel in an institution such as the eponymate? It is hardly conceivable that Bron did neither explain nor clarify all those questions. However, it may well be that all those questions should also be left "à la porte" with the gift! His interpretation is an outstanding example "qui montre une fois de plus la part de subjectivité inhérente à ce genre de procédé" (cf. Aula Orientalis, l.c., p. 138).

The Qat text Ja 3200 may be read as follows:

1 šhr/hll/yhnᶜmʳ/ˀ(bn/y)ʳdᶜᵓl/mkrb/qtbn/bnʳy/ˀwhdt/t[f]

2 ḫmy/gnn/śdw/mᶜn/ḏdwnm/bᶜl/śdw/ywm/rśw/ᶜms/ślttm

1 Šahar Hillal Yuhanᶜim, (son of Ya)daᶜᵓil, unifier of Qatabān, has built and erected the two in[cense]

2 sanctuaries of the garden Śadaw for ᶜAmm, Him of Dawnum, lord of Śadaw, when he was priest of ᶜAmm a third time.

C – J. Ryckmans, the second Qat personal name ᶜrm and Ja 3201.

The second Qat personal name of Ja 350/1, ᶜrm, was the occasion for the "laudemus invicem" color-bearers, A.F.L. Beeston, M. al-Ghūl and J. Ryckmans to really display once more their ugly and irrational argumenting method which is led by "la folle du logis."

Ja 350/1-3 reads as follows: 1 ᵓbqdq/ᶜrm/whbᵓl/ḏhrn

2 wddrᵓn/sqnyt/ᵓnby/śym

3 n/ – – –

1 ᵓAbqaduq ᶜArim [celle de la fʃ mille de] Wahabᵓil, celui (du clan) de

Hirrān

2 et celui [du clan] de Ḏaraᵓān, a dédié à ᵓAnbay Le Pa-

3 tron, – – – (cf. my Pièces épigraphiques de Ḥeid bin ᶜAqīl, la nécropole de Timnaᶜ (Haǧr Kohlān), Louvain, 1952, p. 194.

1 – A.F.L. Beeston.

Beeston devotes almost 1 1/2 pages of Le Muséon, 94 (1981), pp. 59–60, to his discussion of ᶜrm.

a – Beeston's initial paragraph explains his own interpretation in a tirade which, although rather long, is reproduced below in its entirety because such a pearl is too beautiful to be cut. It reads as follows on p. 59:

> "The relationship that I would suggest is the hitherto unrecorded one of «widow» or «relict». In formal English a widow is styled the 'relict' of her former husband because she has been deprived of his protection: and the term has no masculine counterpart because a man whose wife has died has not been deprived of any protection. In mediaeval European law, a woman whose husband is still alive is a 'femme couverte', i.e. covered or protected by him. These parallels would allow us to envisage *ʿrm* as related to Hebrew *ʿarom* «naked, uncovered» and Arabic *ʿarama* «strip (a bone of its meat, a branch of its leaves)». The dedicant of Ja 350 is, I would suggest, described as «the relict of Wahabʾil».»

The first criticism is that the word "relict" is not at all "formal English" but an ordinary legal term still found in the latest abbreviated dictionaries. The second criticism deals with Beeston's argumenting method. He starts his argumentation on the wrong end, thus proving without the shadow of a doubt the apriorism and preconception of his reasoning, a processus unacceptable in scientific research. On the basis of two features, viz. a word of his "formal English" and a point of "mediaeval European law," two realities having nothing whatsoever to do with SA, Beeston anachronically injects into both an Hebrew adjective and an Ar root a derivative meaning which remain unattested in either. It is immediately obvious that such a mixture of so ill-assorted bedfellows in a single quadriga is so repulsive to scientific methodology, is such "a misuse of the etymological tool" (cf. A.F.L. Beeston himself in Le Muséon, 85 [1972], p. 537) that it may safely be denounced as crazy. The drastic difference of institutional and social behavior in the three civilizations makes it certain that Beeston's transfer is a repugnant abuse which his well-known pan-ar alone should even at first sight have rejected. In my study (cf. JaMAR, XV, pp. 56–61) of Beeston's review (cf. JSS, 27 [1982], pp. 101–108) of a book written by J. Pirenne, I quote his verdict which reads as follows: "in dealing with the linguistic part of her work

.... she [= Pirenne] displays a lack of sound philological method which is little short of disastrous" (p. 101). The preceding example of ʿrm can hardly be clearer than it is: Beeston's work is even worse than Pirenne's because the latter at least juggles and outstretches Ar words; Beeston, on the contrary, imposes upon an Hebrew adjective and an Ar root a meaning created at least 8 (eight) centuries after the beginning of Islam.

b – Parallelly to his above-mentioned interpretation, Beeston states that "Worth mentioning is an alternative proposal put forward by M. al-Ghul, of taking the word as meaning «foster-mother»"

apparently on the basis of an Ar expression mentioning ʿarim = ʿârim. Ghul's argumentation is preposterous. Beeston reproduces the Ar phrase that is found in poetry (not in the ordinary language) in this phrase:

> "a verse cited in the *Lisân* (s.v.) describing a woman who has too much milk for her one child, *ka'annnī l-yabiyyi lā law tajid 'ārmān ta'ārum* «like the mother of one child who, if she does not find a foster-child (to suckle in addition), milks her own breast»."

Lisân's texts reads as follows (XII, p. 395B):

" ولا تَلْتَفِتَنْ كأمّ الغلا
مِ ، إن لم تَجِدْ عارِماً تَعْتَرِمْ

يقول : إن لم تَجِدْ من تُرْضِعُه دَرَّتْ هي فحلبت
ثَدْيَيْها ، وربما رَضَعَتْهُ ثم مَجَّتْه مِنْ فيها ؛ "

(1) Beeston's copy substitutes ġabîy to the original ġulâm. If the error is Ghul's, Beeston is nonetheless guilty of gullibility for endoring Ghul's copy without checking it against the original.

(2) Ar ġabîy, as ġulâm, means "male child, boy" and not simply "child," as Beeston puts it.

(3) The Ar phrase proves exclusively that ʿârim means a sucker of the breast (cf. LaAEL, p. 2025C), which is quite normal since ʿarama means "to suck the breast."

(4) The meaning of the 8th form of ʿarama is NOT "to suckle," but only "(said of a mother) to seek one who would suck her breast; or to suck the milk of her own breasts" (cf. LaAEL, p. 2025A).

(5) The transfer of the meaning of the reflexive verbal form ʾitʿarama to the direct transitive meaning of "foster-mother" is a gross error. Beeston's deduction from the expression is that "it implies a passive sense for the Qat noun, 'one who is suckled'." The transfer of the reflexive meaning of ʾitʿamara to a passive meaning, "one who is suckled," is as preposterous as Ghul's. If Beeston, putting aside the 8th form ʾitʿamara, considers the 1st form of ʿarama, then the noun should be the passive participle maʿrûmat. But, the text has ʿrm, not "mʿrmt." Furthermore, Beeston's suggestion can only be described as insane. Indeed, the Qat expression ʾbqdq/ʿrm/whbʾl of Ja 350/1-2 would mean ʾAbqaduq, who is suckled, Wahabʾil, since there is no preposition between ʿrm and whbʾl.

c – Then, Beeston attempts to explain the acsence of the feminine ending –t by invoking two Ar parallels (p. 60):

"C 523/4 *nfs'* = *nafsā'* «woman in childbed».
Ja 623bis/3 *grḥ* = *jarḥā* collective «wounded»."

This argumentation makes no sense unless Beeston abandons his "passive sense" for the active. But, as already pointed out above, the meaning of Ar ʿarama is "to suck," not "to suckle." Furthermore, his two examples are erroneous.

(1) Ja "623bis/3" is an error instead of 643bis/3.

(2) Even taking for granted that the equivalent is proven as a fact (it is erroneous, cf. immediately below), Beeston's claim that ʿrm "might correspond to an Ar fvʿlā [sic!] or fvʿlāʾ [sic!] pattern (p. 60) would still be erroneous: Beeston should have proven – and he cannot do it – that those two Ar forms were inherited from SA instead of being properly Ar.

(3) The equivalence of SA "nfsʾ = Ar nafsāʾ" (cf. also SDDS, p. 93) is the result of a biased aprioriism because "a woman in a state following childbirth" is rendered in Ar by both Ar nufasāʾ = nafasāʾ = nafsāʾ AND nâfis (cf. LaAEL, p. 2829A).

(4) The equivalence of SA "grḥ = jarḥā collective 'wonded'" also is erroneous based, as it is, on Beeston's misunderstanding of Ar "jarḥā جَرْحَى, which is the plural of jarîḥ (cf. FrLAL, I, p. 263A). But, here again, Beeston's equivalence is the result of a biased aprioriism because "jirâḥ is a coll[ective] gen[eric] n[oun]" (cf. LaAEL, p. 405B and xxix for the abbreviations). SDDS (p. 50) refers only to "jarîḥ" which, as said before, is the singular of jarḥā.

Beeston concludes his masterly argumentation saying that since his two preceding – erroneous – parallels are "rare," "I would prefer one of the two other explanations." Since none of the two interpretations suggested by his

is acceptable. Beeston in fact wasted more than a full page of Le Muséon.

Beeston's opinion was endorsed by S.D. Ricks in his thesis entitled A Lexicon of Epigraphic Qatabanian (Ann Arbor, 1982, pp. 187-188). For my review of this thesis, cf. JaMAR, XIV, pp. 62-63. In this particular case, Rick's summary beheads Beeston's opinion by eliminating any allusion to both the "formal english" and the "mediaeval European law," thus depriving of its basis Beeston's fallacious interpretation of both the Hebrew adjective and the Ar vert.

Additional remarks on grḥ of Ja 643bis/3.

a - The idea of "wounded" in the text is preposterous because the end of l.2 makes sure that the authors itemize what "they took away and carried away." Who in his right mind would even think that they would have taken and carried away with them "wounded," whether animals or men? It should, however, be kept in mind that, according to Robin (cf. above, p. 78), a Sab army actually took back home a booty consisting of "men and horses, alive and killed"!!!

b - W.W. Müller would prefer referring grḥ to ʾath. (wohl aus dem Amharischen) gārəḥ 'abgerichtetes Pferd" and, most probably because of ʾfrs horses are already listed at the end of l. 2, he translates grḥ as "Haustier (wörtl.: zahme Tier)" (cf. AfO, 21 [1966], p. 107A). First, his transition from "horse" to "domestic animal" cannot be justifed on the basis of the Ge'ez parallel cited (cf. gərḥ "tamed (horse, mule)" in W. Leslau, Comparative Dictionary of Ge'ez (Classical Ethiopic), Wiesbaden, 1987, p. 202B) because "Haustier" also includes the bovidae. Müller's transition is imposed upon him by ʾfrs, which already by itself contradicts his etymology. Furthermore, since Sa fras never refers to a wild horse, but always to a domesticated one, grḥ would be a tautology; so Müller's etymology is rejected even at first sight. Therefore, I maintain my etymology which calls for "Ar ġāriḥat, plural ġawāriḥu 'carnivorous animal: hunting animal or bird" (cf. JaSIMB, p. 145B).

2 - J. Ryckmans.

Recently J. Ryckmans came on stage to deliver his message on the question. If he is not confident of having said the last word at least according to his phraseology, his presentation is probably nothing but a smoke screen. Whatever the situation may be, he at the very least feels certain that he has discovered the scissors with which the gordian knot may be cut. He takes up the question of ʿrn in his publication of RycISA 2 (cf. Sayhadica, pp. 173-175), which is republished here as Ja 5201.

Taken in by C. Robin's whirlwind to which he is actively collaborating,

Ryckmans copies the latter's fancy way of listing the texts. However, to create of making his reader believe that he is not a mere plagiarist, Ryckmans inserts the initial of the first name of the person who sent him the material: thus, his siglum is "JR Wbrashear."

It is manifest even at first sight that Ryckmans intends to convey the overpowering impression that his paper is up-to-date and nothing but the very best. In fact, however, it is nothing but a fake window dressing aimed at throwing dust in the reader's eyes.

a - Ryckmans speaks of "la face supérieure, visible sur la photographie" (p. 173). This is not true: nothing of the upper side is visible on the photo he publishes.

b - Ryckmans never refers even once to Beeston's study of Ja 550, which is exclusively devoted to the word ʾrn.

c - He states that "outre notre texte [= RycISA 1], deux autres inscriptions attestent la graphie de l'époque de Kmfʿnm: YC 1001 et Ja 2473" (p. 166). Thus, Ryckmans eliminates the following texts:

(1) Ja 674 and 675 a-k which I published in BASOR, No. 138, April, 1955, pp. 46 (with photo of the text),47, respectively;

(2) TTI 656 published in JaMAR, IX, pp. 52-53 and photo on pl. 5; and

(3) Van Beek's text in The Biblical Archaeologist, 15, February, 1952, no. 1, p. 1.

d - On hwfʿn/yhnʿn's genealogy, Ryckmans states that "cette suite généalogique est établie par les données des textes Ja 119, 121 et 122" (p. 166). This statement and the corresponding footnote 3 read as if Ryckmans himself had built up that genealogy. Yet, I have established that genealogy in R. LeBaron Bowen, Jr., et al., Archaeological Discoveries in South Arabia, Baltimore, MD, 1958, p. 192. Two more small remarks: in footnote 3 (p. 166), correct "198" to 193; and on p. 180, l. 8, "in 1950-1951" is a part of the title and must be underlined.

e - Ryckmans' etymology of ʿrn is absolutely insane.

Ja 5201 reads as follows: 1 ...]ʿrn/ḏn[...
2 ...]ʿthn,ḥn,hw[...

which Ryckmans restores as follows:

"1 [. .] ᶜrm šh[r
2 [Ygl Yhrgb mlk Q]tbn bn Hw[fᶜm Yhnᶜm" (p. 173).
"1 [Mémorial (?) de N N], gage (?) de Šah[r
2 [Yaqul Yuhargib, roi de Qs]tabān, fils de Haw[fᶜamm Yu-
hanᶜim" (p. 174).

(1) Ryckmans declares (p. 175) that

"La distribution en deux lignes du texte restitué est évidemment donnée à titre d'exemple; elle est présentée ici en tenant compte de la longueur supposée de la lacune de la ligne 1, qui comportait sans doute le nom de l'objet, et la dénomination de la personne qualifiée de ᶜrm."

Yes, it is an example – and, indeed, a very good one – of a restoration which even at first sight is unacceptable , based as it is not only on the aprioristic idea that l. 1 begins with mᶜmr but also on the gratuitous assumption that šh[must be restored as "šh[r]" which, in turn, results from the aprioristic idea that the same šh[is connected with the royal titulature of which remain seven letters only in l. 2.

(2) L. 1 would be 12 letters shorter than l. 2; such a feature is wholly unacceptable.

(3) According to Ryckmans' "exemple" of restoration, there would be 10 letters only missing at the beginning of l. 1, including his "mᶜmr." There would, thus, be only 6 letters for the two names of the author, "N N," which is impossible.

(4) Ryckmans distorts the epigraphical evidence by placing š of šh[, the 6th sign of l. 1, immediately above t, the first sign of l. 2, instead of above the space between bn/ and hw of l. 2. This initiative is deliberate because it is aimed at nicely forcing the disposition of the letters on the fragment so as to render evident his restoration of "la lacune de la ligne 1, qui comportait sans doute [= without any doubt] le nom de l'objet," – "qui était peut-être un mᶜmr" – "et la dénomination de la personne qualifiée de ᶜrm."

(5) In fact, the letters being reproduced – as it should be – as they appear on the fragment, Ryckmans' restoration should have been as follows:

1 [12 letters]/ᶜrm/šh[r]
2 [ygl/yhrgb/mlk/q]ᶜtbn/bn/hw[fᶜm/yhnᶜm]

Such a restoration also is unacceptable because l. 1 would still be eight letters shorter than l.2. Therefore, the connection of šh[with the seven letters of l.2 does not exist and should never have been suggested. This is all the more obvious in that šh[does not necessarily have to be restored as šh[r], contrary to Ryck-

mans' verdict, "ᶜrm est suivi de šhr" (p. 175). Finally, the two royal names suggested by Ryckmans must be maintained because they are the only ones that fit the seven remaining letters of l. 2.

As an introduction to his study of the text itself, Ryckmans delivers his comments on two other texts.

(a) He states (pp.174-175): "Il faut ici rapprocher du texte Ja 350 [2], où une femme, 'bsdq ᶜrm whb'l ḏhrn wddr'n, offre (sqnyt) son mᶜmr. A. Jamme considère que cette formule est mise pour 'bsdq ᶜrm [ḏt byt (ou :ḏt)] whb'l ḏhrn etc. Cette interprétation nous paraît en soi peu plausible, et peu satisfaisante si on la compare au présent texte."

What can possibly be the meaning of this verbiage as it stands? What does he intend to prove? The two expressions are, for him, absolutely identical, viz.

Ja 350/1: ʾbsdq/ᶜm/whbʾl/ – and his restoration of the present text has /ᶜrm/šh[r, how can he use the second pericope to reject my interpretation of the first which is based on both the formulas used in the Qat proscynemas and the fact that ʾbsdq is feminine because of the feminine verbal form sqnyt? To find the exact meaning of his statements, the reader has to keep on reading.

(b) The second text is Ja 864. Ryckmans writes this (p. 175):

"Dans Ja 864 [3] :Ihy'b ᶜrm ᶜmrm, que Jamme lit :"Tahay'ab ᶜArim [de la famille] ᶜAmrum", subsiste la même difficulté de considérer ᶜrm comme une épithète de femme (la statuette qui porte l'inscription est féminine)."

First of all, it should be noted that his bibliography (p. 180) fails to mention the plate of the tracings in the title of the thin brochure where the text is published. At least in the present case, Ryckmans' wording is clear, and the reader cannot but wonder why he made his confusing statements in the preceding example.

Ryckmans' treatment of the crucial section of his paper which is his interpretation of ᶜrm is so preposterous that it has to be reproduced here in its entirety because the reader can hardly believe it.

"A notre avis, et [A][B]
en tout cas dans Ja 350 et dans notre texte, ᶜrm doit être un substantif
régissant le prénom masculin qui suit, terme abstrait (puisqu'il peut [C]
viser des femmes sans prendre la marque du féminin) indiquant un état de
sujétion sociale à l'égard de cette personne. En évoquant (mais sans [D][a]
prétendre à une équivalence étymologique) le mot sud-arabe ᶜrbt
au sens de "gages, otages", et la racine arabe ġarima, qui donne par [b]
exemple le mot ġarīm, "créancier" ou "débiteur", et enfin en songeant au [c]
sens donné à lw', "gage" (s'agissant de personnes) dans les inscriptions

mindennes [4], nous présentons, à titre de conjecture, l'interprétation "gage (?)", en rapportant ce terme (comme dans Ja 350 et 864) à une personne (ici mentionnée dans la lacune), et non à l'objet de la dédicace, qui était peut-être un m^cmc" (p. 175). [E] [F]

Ad [A] – The most important element of the thesis is "à notre avis" which plays the role of a pacifier introducing the whole saga which, in turn, will remain without any proof and is built on the craziest argument I have ever seen.

Ad [B] – Although Ja 864 offers exactly the same expression as Ja 350, the first is excluded from his argument which involves only Ja 350 and the present text. Here again, Ryckmans does not explicit his reason or reasons for excluding Ja 864. Furthermore, since there is absolutely nothing in the 12 letters of the present text which might even very remotely be connected with the feminine gender, his "en tout cas" remains unjustified and doubtlessly is another element of his aprioristic autosuggestion. Summarily, Ryckmans's attitude is incomprehensible: he excludes one case which is clear but takes a firm stand on the other where there is no evidence in one way or the other. However, since the author is Ryckmans, the reader can always expect something abnormal. And he does it again here: he contradicts himself openly by explicitely reinstating Ja 864 alongside Ja 350 in his [E].

Ad [C] – The expression "terme abstrait" is nothing but an "ens rationis" or an imaginary being, and can evidently not be applied in the contexts involved which would demand a concrete noun, such as ʿbd for a man or ʾmt for a woman. It is because of this reason that Beeston sought in vain to find examples of SA words representing the Ar feminine derivations fʿlā and fʿlá (ay). But his examples are invalid. Furthermore, Ryckmans is also wrong when he gives the double gender of a word as the justification of it being "abstrait." These are two different realities; cf., e.g., concrete (not at all "abstrait") nouns designating individuals and of both genders, such as "architecte, esclave, partenaire," etc. The noun "gage (?)," his translation of ʿrm, is a concrete noun, not an "abstrait" one.

Ad [D] – Ryckmans' triptich-like etymological argument is so fantastic that it defies any qualification. But, since it is done in cool blood, doubtlessly after protracted deliberations and presented in a supposedly scholarly publication, the reader, keeping in mind the author's manifest errors already denounced in the preceding pages, cannot but apply the axiom "ab uno disce omnes" and consider Ryckmans as a resolute blunderer so blinded by his own preconceptions that he does not even shrink from denouncing J. Pirenne's "graves déficiences philologiques et méthodologiques" (cf. Le Muséon, 91 [1978], p. 253). At least, Pirenne seeks the so-called proof of her aprioristic ideas in Ar derivatives of the same root as the SA word. Ryckmans does not even take that elementary precaution. Here is a summary of Ryckmans' genial argumentation of which even the phraseology belongs to the world of fortune-telling, viz.

[Da] "en évoquant ... ʿrbt ... et [Db] ġrm, ... et
[Dc] enfin en songeant ... à lwʾ" (p. 175)?

It should be noted first that in Ryckmans' transcription of the title of R. de Vaux's contribution listed in his footnote 4 (p. 175) the second mention of "lévites" is erroneously put between two quotation marks which must be deleted: they do not exist in either volume where that contribution appeared. Furthermore, it is unfortunate that Ryckmans did not read p. 278 to which he refers (= p. 266 of the editio princeps) because that page reminds him that the title lwʾ is masculine, but lwʾt feminine.

Ryckmans' triptich clearly proves from a SA viewpoint that he started his whole reasoning with a preconceived idea given to him by the Kin word lwʾ which he, in turn, rather easily connected with Sab ʿrbt. Then, he searched for a possible etymology for SA ʿrm. Not finding any under ʿrm which could possibly satisfy his aprioristic idea, he searched under ġrm as the consonantic equivalence SA ʿ and Ar ġ is sometimes valid. But the existence of this consonantic correspondence makes it incomprehensible that Ryckmans did not give his argumentation some appearance of rationability by starting it with ġarīm and his crooked mind would surely have had no major difficulty in squeezing in the relationship creditor-debtor his aprioristic idea of "pledge;" viz. it is completely incomprehensible – at least to me – to see him preferring an argumentation which even at first sight is repulsive.

Ryckmans' starting point, ʿrbt [Da], has nothing whatsoever to do with the word ʿrm except for the material identity of ʿr as the first two consonants of either root. If such a starting point does not prove the author's aprioristic approach to problems, nothing will. Furthermore, [Db] Ar ġarima or ġarīm cannot have any relation with ʿrm of the present texts because ġarima means "to pay, discharge" and, contrary to Ryckmans' statement, ġarīm's meaning is "debtor," but also sometimes a "creditor" (cf. LaAEL, p. 2252C, and Ar ġarima "to pay, discharge," and also FrLAL, III, p. 272A). How could Ryckmans even think of the possibility of a SA woman declaring herself the "créancier" of her king?

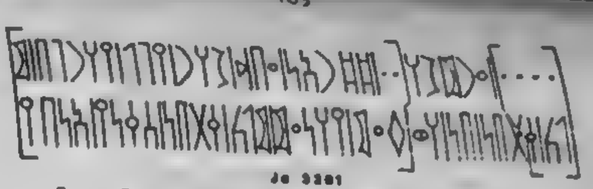
Ad [Dc] – The word lw³ is so different from ʿrm – not even a single consonant is common to both – that only a blinding aprioristic idea can possibly bring it up. It should also be noted that Ryckmans does not refer to SDDS on the question of ʿrbt. SDDS has this on the subject: "n. tʿrb Ja 578/22, p. ʿrbt Ja 574/11 pledge, guarantee of good conduct" (cp. 19). It is hardly conceivable that ʿrbt be considered as the plural of tʿrb, and there is no proof at all that ʿrbt is a plural. JaSDDB explains the difference between the two words, viz. "ʿrbt pledge, 574/11" (p. 445B) and "tʿrb 'to give pledges of submission;'n [inf.] 735/9; [maqdar] 578/22" (p. 450B).

Ad [E] – Ryckmans accurately presents his translation of ʿrm as a "conjecture," viz. an inference from nonexistence.

Af [F] – Ryckmans breaks into an open door by pointing out that ʿrm is related to a person: this connection is so evident in the text that it is quite superfluous to mention it.

Last, but not least, as the consonantic equivalence means nothing to Ryckmans, why did he not explain SA ʿrm "en évoquant (mais sans prétendre à une équivalence étymologique)" the Ar nouns ʿarûs or haram (or harîm)? After all, it is only a s instead of m, or a ḥ instead of ʿ. Either word would provide him with most appealing translations, viz. "bride" or "wife." Being feminine by signification, either noun would eliminate the search for an explanation of the absence of any feminine ending.

3 – My restoration of the fragment RycISA 2, which is here republished as Ja 3201, takes into account not only Ryckmans' errors denounced in the preceding pages, but also the fact that šḥ[may be restored, e.g., either šḥwy of Ja 902A/3 or šḥrn of the fake Honeyman 5/1, 4 and 6 inasmuch as this name may represent a genuine original. Of course, it is only an attempt and its value does not exceed the mere possibility. However, the number of the letters in l. 1 is only 31, but 33 in l.2. Such a feature is not accidental, but was devised to take into account that the letters of l. 2 are slightly more crowded than those of l. 1, viz. the width of the 7 signs of l. 1 equals that of the 7 1/2 signs of l.2. Finally, my restoration also heeds the absence of any clue as to the femininity of the author. Would any fragment show up and contain that indication, ddr³n, ʿbd and sqny could easily be changed to dt/dr³n, ³mt and sqnyt, respectively. The whole Qat documentation now at our disposal does not support such a possibility.



Ja 3201

1 [....]/ʿrm/šḥ[../ddr³n/ʿbd/šhr/ygl/yhrgb/m]

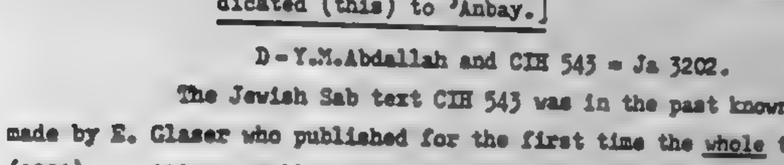
2 [lk/q]ᵀtbn/bn/hw[fʿm/yhnʿm/mlk/qtbn/sqny/³nby]

1 [....] ʿArim [of the family of] Šaḥ[.., he of (the clan of) Dara³an, servant of Šahar Yagul Yuhargib,]

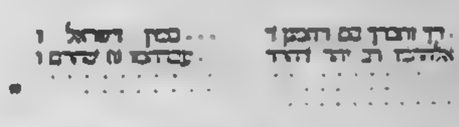
2 [king of Qa]tabân, son of Haw[fʿamm Yuhanʿim, king of Qatabân, has dedicated (this) to ³Anbay.]

D – Y.M.Abdallah and CIH 543 = Ja 3202.

The Jewish Sab text CIH 543 was in the past known only by a copy made by E. Glaser who published for the first time the whole text in REJ, 23 (1891), p. 122, as follows:



It should be pointed out immediately that the text has wkl/bh.h in l. 5, not "wk/lbh.h," as in CIH's transcription in SA letters. Glaser had earlier the same year given his transcription of ll. 1-2 only in Das Ausland, 64 (1891), p. 28B, as follows:



Glaser does not reproduce the word dividers; neither does he separate the words as in the text; e.g., he transcribes "rb yhd" (l. 2) instead of rbyhd. In his publication of the whole text, he wrote the following statement:

> "Ma copie porte רבירוד en un seul mot, et non רב | ירוד, en deux mots séparés par un trait vertical, ce qu'exigerait, semble-t-il, l'usage sabéen, si l'on devait lire רב ירוד. "

The fact remains that it is precisely "rb yhd" that his first transcription has.

It is Glaser's text of REJ that H. Winckler reproduces in his Alt-

Orientalische Forschungen, IV (1896 = 1972 reprint, Reihe I), p. 335, viz.

" [illegible]

Winckler altered Glaser's copy in four places; viz. l. 1:"[b]rk" instead of ".rk;" l.1: "ysr'l" instead of "yśr'l;" l. 3: "[']mhw" instead of ".mhw;" and l. 5: "b[yt]h[w" instead of "bh.h." Even in Winckler's transliteration, wkl of l. 5 is certain. Yet, the facsimile after p. 336, apparently from Glaser's hand, clearly has "wk/l," which was repeated by CIH (II, p. 299) in his transcription in SA letters.

In Sayhadica, pp. 3-8 and 2 good photos in pl. 1, Y.M. Abdallah republishes the whole text on the basis of the two original fragments which ascertain the reading of the whole inscription. The two fragments are now at the Ẓafâr Museum under the siglum "ZM 772A+B."

Here follow some remarks on the author's reading and commentary.

Abdallah correctly states that J. Halévy wrote "three short articles" in connection with Glaser's papers (p. 3), but his footnote 1 omits the reference to the second article, viz. pp. 280-281. — He also correctly affirms that "the ṣ is certain" in aṣrm of ll. 4-5 (p. 5), but "the name is dubious and may be a corruption of 'ṣrm" (p. 7). These two statements which remain without any explanation, justification or commentary, are unacceptable. — The author's correction of "b.m" to bdm (p. 5) is on l. 3, not l. 2. — He also states that "the l of kl is certain" in l. 5 (p. 5). But, as shown above, the letter has never been in doubt. — The author also affirms: "instead of bthmw, read bhth" in l. 5 (p. 5). The origin of his "bthmw" remains unknown. As shown above, Winckler has "b[y]th[w].

The following corrections should be noted in the reedition.

1 - ʿbdhmw (l. 2) is translated as "his servant" instead of their servant.

2 - 'lwdhmy (ll. 3-4) is rendered as "their children" instead of the children of them both.

3 - The name of śhrm's first child is dmm instead of the author's "dmm" (pp. 5 and 7, and the note on "the d of dmm is certain" in p. 5). Each of the two d's of the text (ll. 1 and 3) has two aslant bars, which is normal; the present letter has only one horizontal bar in the lower half of the letter while each of the seven examples of b has the same horizontal bar in the upper half of the letter.

4 - "kl/bhth" is translated as "all other lowly members of his house." Neither "other" nor "house" are in the text; the author's transliteration "bhth..." should have been "bhth[w/...]" because of the scriptio plena -hw which is attested twice in l. 3. Also, since the second letter of the pronoun -hw is not given in the transliteration, "his" of the translation must be corrected to "hi[s]." Furthermore, since the author of the text mentions 'mhw (l. 3) his mother, bśkthw (l. 3) his wife and then 'lwdhmy (ll. 3-4) the children of them both, viz. of the husband and his wife, it seems normal to restore bhth[my instead of bhth[mw which would include śhrm's mother.

The only word making difficulty is the well-known bht (l. 5). The author bases his argumentation on Ar "ḫutah ... son of a whore" (p. 7), and he concludes saying that "bht means the people in the house who are a part of the family but not of noble birth, illegitimate sons or those who do not belong to the nasab [= kindred consanguinity] of the family" (p. 8). The application of any Islamic social condition or situation to the pre-Islamic society is unacceptable unless there is some evidence in the SA suggesting it. The two cultures are too different from each other to justify it. Indeed, the most obvious objection to the preceding understanding is CIH 604/1: wkl/'nsm/bhtm/wqtnm (cf. also CIH 609/7) which the author translates as "every one (whether he is) of a lowly birth or of a higher one" (p. 8). No justification is offered of the fact that a person "of a lowly birth" would take the precedence over another "of a higher one;" the reverse situation would be expected. The preceding objection must also be applied to Ja 3198/5 which would mean, according to that new opinion, "and all his ones of a lowly birth and their brothers" (cf. above, p. 54).

A.F.L. Beeston suggested in 1979 - thus, during the preparatory stage of his SDDS - that "there is some attraction here in a proposal made by M. al-Ghul, that this formulaic expression [= bht followed by qtn] means 'transient or resident'" (cf. Raydān, 2 [1979], p. 89), with reference to endnote 2 (p. 99): "Based mainly on Ar qaṭana 'reside'." The guessing attitude of those authors is illustrated by the fact that in SDDS (p. 27) the preceding suggestion is forgotten without even being mentioned and is replaced by two translations, "great" and "stranger, alien," the latter being another guessing of Ghul, which is a pure fancy without any etymological justification. In either case, the meanings given to bht describe the physical condition opposite to the meaning of Ar qaṭana "to reside at"

at the expense of the etymology of bht. Yet, ar bahata means "excepit bene et laeto vultu aliquem" (cf. FrLAL, I, p. 164A), and "to hasten to s.o.'s side; to go to meet him"(cf. DAFA, II, p. 865B). No wonder, then, that baht means "alacrity, eagerness; affability" and buhtat "kept woman; concubine" and by "ext.,born out of concubinage" (cf. DAFA, l.c.). Therefore, the latter meaning is nothing but an "ext." (= "by extension use" in I, p.xviB) of a secondary derivation of buhtat "concubine." Note also that FrLAL' first meaning of buhtat is "vacca silvestris" instead of "concubine." The present participle bht, therefore, may be translated as "valet de chambre, waiting man/woman." Then, qtn may be related to ar qāṭin "Sing. et Plur. (et Pl. quṭun) Famuli, servi, ancillae, asseclae, domestici" (cf. FrLAL, III, p. 470A); thus "house-servants," both males and females.

The text may be read as follows:

1 [b]rk/wtbrk/sm/rḥmnn/dbsmyn/wyśr'l/w
2 'lhhmw/rbyhd/ḏhrd'/'bdhmw/šhrm/w
3 'mhw/bdm/whšk thw/šmšm/w'l
4 wdhmy/ḏmm/ mono w'bš'r/wmṣr
5 m/wkl/bht gram [my/...
6 hw/...

1 Be [bl]essed and praised the names of Raḥmanān, Him who [is] in the
heaven, and of Israel, and of
2 their god, the lord of the Jews, Him who has assisted their servant Šahrum and
3 his mother Baddum and his wife Šamsum and the
4 children of them both Ḏammâm and mono gram 'Abša'ir and Muṣ-
5 rim and all the valets of th[em both...
6 his ...

L. 1: the two forms of the verb brk have the same meaning; cf. Ar tabâraka "to be blessed"(cf. J. Penrice, A Dictionary and Glossary of the Ḳor-ân, New York, 1969, p. 16B).

L. 1-2: sm is a construct plural. There is no justification why the name of rḥmnn alone would "be blessed and praised" while Israel is mentioned in the same prayer and all the more so because sm as a singular would not affect 'lhhmw which stands for rḥmnn. The two lines make it clear also that for šhrm, the author of the text, rḥmnn is at the same time the god of the cosmos in general and of Israel in particular. — 'bdhmw: although 'dm would have been expected instead of 'bd as the noun introducing at least nine persons, 'bd was retained to indicate that šhrm, the head of the family and the first of all the persons mentioned, really is the author of every activity of his household and, therefore, the actual receiver of every God's favor bestowed upon that family. The persons. pronoun -hmw is a plural of majesty because rḥmnn actually is the first and main object of thanksgiving for hrd' He [= Raḥmanān, and He alone] has assisted the author and all his relatives and dependents listed in the text either directly or indirectly through yśr'l.

L. 3: the author does not give any reference to bdm; cf. Ja 2478b and 2570a. — šmšm: the author's commentary speaks only of "names constructed with the divine name šms;" for šmšm as a second masculine name, cf. CIH 43/1.

L. 4: dmm, cf. Ar dammâm "who collects together the seed-produce" (cf. LaAEL, p. 1801B). — 'bš'r: this name might be attested in RÉS 2740/5 if M. Hartmann's correction of J. Halévy's copy "'bšwr" is correct (cf. Die arabische Frage, Berlin, 1909, p. 179). A. Capuzzi reproduces the text in her Iscrizioni minee (Naples, 1974, p. 3), where the name is given as "'bš'r," and she claims that her transliteration is "Lettura: RES." In RÉS (V [1928], pp. 93 and 94), the name is clear: "'bš(')r."

Ll. 4-5: the monogram is referred to in the author's transliteration as "mono- gram"" (p. 5), and the asterisk has "The monogram reads šhrm."

E – Ja 1093.

M. Höfner republishes a text from 'Aryab on pp. 37-39 and photo on p. 47A. Unfortunately, she is not aware that the text had already been republished 17 years ago as Ja 1093 in JaMAR, II, pp. 91-92 and facsimile in pl. 7, and that the following year, JaMAR, III, p. 90, had also listed the corrections to be made to DoSA, p. 167. Since Höfner's reading and translation do not differ from mine, her paper is hardly justified. One remark, however, is added here. Höfner writes that "g und l sind in der Schrift praktisch nicht zu unterscheiden" (p.38). This statement is erroneous: the upper left extremity of the appendix of g reaches the upper line of the letter space; that of l does not. It is most amazing that the confusion of g and l is repeated at saturation. JaMAR, XIV, pp. 75-78, took up the question once more with examples from Beeston, Robin and Ryckmans, and their confusion triggers them almost instantaneously to the correction of the letter they do not like. A good example of their erroneous reading of g instead of l is l'wrm of Gr 3/1 is given on p. 76. In Ṣayhadica (p. 119), Robin gives another example of his ignorance presented ... in a scholarly manner as follows: "la deuxiè-

me et la troisième lettre peuvent être lues lâm ou ǵim. Dans la mesure où l'inclination de l'appendice semble plus marquée dans la troisième lettre, nous retenons provisoirement Wgl" of Ir 40/7. There is no question at all of "semble" or "provisoirement;" the epigraphical fact is there and is not subject to any interpretation. A simple glance at the photo makes it clear that the two letters are gl; the difference between the two letters is paralleled by that in Ja 649 (cf. JaSIMB, pl. J). Would all those authors heed the published material instead of going round in the circle of their own preconceived ideas which they rehash without end, they might overcome their ignorance. Finally, yet another example of the very same attitude is given above, pp. 67-68.

It was proper for Ṣayhadica to be offered in homage to A.F.L. Beeston because it too often exhibits the kind of preconceived, irrational approach to problems, argumentations and reasonings which he has been feeding the reader for so long and which is so beautifully illustrated once more in the question of Qat ʿrm not only by Beeston himself (cf. above, pp. 93-96) but also by the master swimmer of the twilight, J. Ryckmans (cf. above, pp. 96-102). It nonetheless is a bittersweat tribute. The preceding pages already pointed out two good reasons. First, Beeston's latest terminological invention of "Sayhadic," which he set his heart on so much that he went as far as to defend it in a special paper, was not endorsed or even referred to by a single of C. Robin's collaborators (cf. above, pp. 44-46). Secondly, even his henchman J. Ryckmans never refers to Beeston's lengthy study of Qat ʿrm (cf. above, pp. 93-96) in his edition of Ja 3201 (cf. above, pp. 96-103). Furthermore, it suffices to read Beeston's expose of his position on ʿrm (cf. his text above, p. 93), in which expressions, such as "I would suggest" (twice) and "would allow" doubtlessly are for the sake of appearances and fool no one, to evaluate C. Robin's toadyish statement, according to which Beeston

> " a toujours grand soin de bien marquer la limite entre les spéculations hasardeuses et les conclusions avérées." (cf. Ṣayhadica, p. XI). S.D. Ricks understood Beeston very well and retains Beeston's translation as a certainty. Robin continues his fulsome flattery by adding that Beeston

> " est d'ailleurs le premier à réviser son jugement avec modestie lorsque de nouvelles découvertes ou un examen plus poussé des documents ruinent sa conjecture." (cf. Ṣayhadica, pp. XI-XII). Fortunately, the reader knows better because he is fully aware of, e.g., Beeston's "spectacular about-face" in the question of the so-called "ritual" hunt, and "the case of the substitution of a text" (cf. JaMAR, XIV, pp. 81-82 and 90-93, respectively).

IV - Two Tham inscriptions from northern Dhofar, Oman, JaT 97 and 98.

It was 25 years ago that JaPRER (p. 46) reported the existence of some Tham rock inscriptions west of al-Wâṣṭah in the upper Wâdî Ḥaḍramawt. Earlier this year, I received from Mr. James R. Jones, to whom my whole gratitude is due, the photos of two Tham rock inscriptions he had discovered in northern Dhofar, Oman. The rectangular form of d with a central horizontal crossbar is common to the two groups of texts.

The importance of those two Tham inscriptions is twofold. Not only they are the first Tham texts to be reported so far in the east, but they also show that, in their eastward migration, the ancestors of the authors of the texts had stayed long enough in contact with the western SA populations to borrow the names ʾwsn and nbṭ as well as the habit of using two-section personal names before getting acquainted later with the wusum used in northern Ḥaḍramawt. It seems possible that a contingent of those Tham migrants remained around a place where a settlement grew up and was called after them, viz. Thamûd about 140 klm north-northeast of Sanâ in Wâdî Ḥaḍramawt, while others pushed eastward for another trek of some 420 klm southeastward to the place where JaT 98 was found, about 37 klm northeast of Salalah, and then northeastward for another hike of some 700 klm to the country of al-Ḥammah, where Ja 97 was recorded.

The following information is given by Jones.

Ja 97 is "roughly 53 x 45 x 10 cm. The stone is siliceous limestone of the Mesozoic Hawasina Complex. Inscribed stone at trilith. Area is Al Hammah. The location is about lat. 22°27'51"N., long. 58°30'49"E. Coordinates: E-06557 - N 24844. Map K 6611, NF 40-8D, I-GSGS, Mintirib 1:100:000. The site is about 30 km west of Al Mintirib, in Sharqiyah. The stone was given to the Ministry of National Heritage and Culture."

Ja 98 "is roughly 65 x 65 x 15 cm. The stone is Eocene limestone. Trilith - downed standing stone near ʿUyūn borehole, Jabal Qara, north slopes, Dhofar. The location is about lat. 17°15'5"N., long. 53°57'30"E. Coordinates: ZV 08146 - 19095. Map K 6611, NE 39-12F, ʿUyūn, I-GSGS. The site is about 28

km north-northwest of the Salalah Civil Aviation terminal.

JaT 97: an upside-down, roughly trapezoidal stone; three vertical lines to be read from left to right and from top to bottom.

JaT 97

1	2	3
ʾ	c	b
w	l	ḍ
s	y	n
n	d	
	d	
	Wasm	

1 ʾAwsān
2 ʿUlayd,
3 he of (the clan)
4 Baḍḍān.
Wasm

ʾwsn, cf. ʾwsy in Hu 123. – ʿlyd: the form of d is already attested in HaLiSTI, pl. 26, letter d, 8th form from the left, which is standing on its left side; a qtyl nominal derivation; cf. ʿld in Ḥāʾil 79, and ʿldd in JsaT 697. – bḍn: HuIR 134 (cf. vdBrIT, p. 480) may be read as follows: bḍ nk syḥ Baḍḍ has cohabited with Sayaḥ. – For the double wasm, cf. JaPRER, p. 47, A I 12 = C I 12a, and A I 4a = C I 4a, respectively.

JaT 98: a smaller, rounded, upside-down trapezoidal limestone; one line to read from top to bottom.

JaT 98

n	w
b	a
ṭ	s
	m

Nābiṭ. – Cf., e.g., Min RÉS 2777/7-8, Qat RÉS 3902, no. 19, and Sab RÉS 4196/4-5. — The upper element of the triple wasm is new; for the composite lower half, cf. JaPRER, p. 47, A I 6b = C I 6b, and C II 13, respectively.

V – Two Tham (JaT 99 and 100) and one Ḥaḍ (Ja 3203) texts from al-Ḥismá, Jordan.

Under the title "Dedanite and Minaean (South Arabian) Inscriptions from the Ḥisma," Jordan, in ADAJ, 27 (1983), pp. 555-569 and pl. 121 and 122, E. F. Graf publishes two texts as "Dedanite" and one as "Minaean" discovered "in the area northeast of Wadi Ram" (p. 555A); "two of the new texts are from a cave called Abu al-Dbaʿ", and the last one was "discovered in the Khrum al-Ghuzlān" (p. 555A). Graf's epigraphical material was gathered during his "archaeological research," which "was made possible by a National Endowment for the Humanities fellowship grant, made available through the American Schools of Oriental Research" (p. 555B). This mine study, which is long overdue, is limited to the epigraphical material.

A – Introduction

1 – All through his paper, the author displays a less than embryonic knowledge of the material involved and also limited to a few publications which are blindly utilized without any critical judgment and from which he extracts a reference here and there. All collected references became the ingredients swimming in the amorphous magma of his ignorance where they take the shapes and the forms imposed by his preconceived ideas and his latius hoc argumenting method. Nothing else can be expected when there is neither proper training nor critical discernment. The author acknowledges (p. 555A)

"the generous and helpful advice of Professors A. F. L. Beeston, W.W. Müller, J. Ryckmans and F. V. Winnett, who greatly facilitated my entry into the field of pre-Islamic epigraphy."

However, since it is well-known not only that none of the first three persons listed is a specialist in NA (as demonstrated in JaMAR, XIV, pp. 62-259) but also that F.V. Winnett's work is plagued with all kinds of inconsistencies and extravagant theories suggested for the sake of the cause (cf. my review of his last paper printed in JAOS, 117 [1987], pp. 239-244, cf. JaMAR, XV, pp. 124-191), Graf's so-called initiation could hardly have been worse, and the result is here for everyone to admire and has the same value as that of J. Koenig's presentation of the epigraphical material from al-Jaw (northwestern Saudi Arabia; cf. his booklet

entitled Le site de al-Jaw dans l'ancien pays de Midian, Paris, 1971, and my republication of the epigraphical material in al-Mudhud. Festschrift Maria Höfner zum 80. Geburtstag, Graz, 1981, pp. 99-105, 108-109 and tracings in pl. 2), viz. the effect of burdening scientific research with too many undesirable hypotheses written by an unqualified author.

2 - The tracings of the inscriptions were made by "Ms. Christine Leonard" (p. 555B). On the one hand, as the author does not specify her qualifications and the tracing of the second text utilizes a decorative stippling as letter filling, the reader assumes her to be a draftwoman. On the other hand, since the facsimiles concord perfectly with the author's readings, the value of her work is to faithfully reproduce the author's readings which according to his own testimony, "are primarily based on the copies made at the time of recording" (p. 555A).

The exact meaning of his "primarily," however, remains unknown because the author does not give a single example of a retouch of his initial copies. Whatever the extent and the location(s) of the emendation(s) may be, the author is responsible for the letter forms of the facsimiles because they doubtlessly conform perfectly with his own copies.

3 - The basic question involved by any text is its alphabet and language. The whole paper from title to end makes it abundantly clear that the first two texts are declared to be "Dedanite" (cf. the title) or "may be classified as Dedanite" (p. 555A), and "the other is a Minaean graffito" (p.555A).

a - The initial remark to be emphasized is that "Dedanite" is a geographic appellation applied to the ancient city of Dedan and its surrounding area. The ancient languages spoken there were the native Lih and the imported Min, Than and Nabataean. Cf. below, p. 300.

b - The first two inscriptions.

(1) The author's ascription of these two rock inscriptions to his "Dedanite" is based on their "script, which is distinguished from Lihyanite by the circular form of certain letters (like the *y*, *w* and *y*) and the general uprightness of the script (Caskel 1953: 23 and WLT 10)" (p. 562A). The first publication mentioned, Call, was published in 1954 (not in "1955"), and the page referred-to by the author is 33 (not "23"); the first of these two errors is also repeated in the bibliography in p. 568, where two more errors are found, viz. "Liyanisch" and "Arbeits-gemeinschaft" instead of "Lihyanisch" and "Arbeitsgemeinschaft" (in one word). Furthermore, it is childish and ridiculous to give so much authority to Call who dramatically shematizes the examples of his choice. As for the second publication, viz. WiSLTI, Graf is so blinded by his preconceived idea that he did not even bother to take a good look at the chart of pl. 10 where the rounded forms of his four selected letters are found in both "Dedanite" and Lih. On the "general uprightness of the script," only an ignorant would exclude that ductus from Lih. Had the author looked, e.g., at JaNAR, VI and VII, where so many tracings of Lih texts are published, he would have seen - it is to be hoped - how preposterous his whole statement really is.

(2) As some kind of confirmation of his basic statement, the author adds this (p. 562A): "Of note are the peculiar Dedanite *g* in *'bgr* (no. 1) and the enclosed base of the *m* and the vertical *y* in *mr* (no. 2), which stand in contrast to the form of these letters in Lihyanite." The only effect of the preceding statement is to add another proof of the author's ignorance. - The first sign is already shown as a characteristic of early Than in WiSLTI's pl. 10: "Thamudic A;" it is also found very often in Lih (e.g., JaL 8c, e, f, etc) as well as in Than (e.g., JaT 20a, b, 22, etc.). - The form of *y* also is well-known in Lih (e.g., JaL 8b/2, d/2, and Call, p. 33) as well as in Than (e.g., Hu 326, JaT 38d, etc).

(3) The first two inscriptions manifestly belong to Than because of the sturdiness and the width of the letter forms.

c - The author states that the third inscription "may be considered Minaean for the reasons stated above" (p. 562B), and the text is labeled "The Minaean Graffito" (p. 562B).

(1) Since the author himself declares the text to be "engraved in monumental South Arabian style" (p. 562B), he is contradicting himself when calling it a "graffito" which necessarily implies cursive lettering.

(2) The so-called "reasons stated above" do not exist. The only fact mentioned "above" by the author is the Min influence on his "Dedanite" (p. 562A) as if it was not a well-known fact that the Minaeans influenced the Lih spoken in their northern colony of Dedan.

(3) The author's survey of northern Min is so embryonic that he even characterises A.F.L. Beeston's study of two Min texts in Bulletin of the Institute of Archaeology (London), 10 (1972), pp. 52-55 as covering "the most recent additions" (p. 563A). The author does not

even know that these two texts are engraved on the very same stone and that they were republished in 1974 in JaMAR, IX (pp. 66-7 and pl. 17) along with other Min inscriptions.

(4) The author crowns his erroneous presentation of northern Min with these two statements (p. 563A):

> " it appears that the South Arabian clay stamp found at Bethel, which has been mired in controversy, is clearly out of context and the initial suspicions about the stamp as a modern intrusion that were expressed appear to be justified (Cleveland, 1973)."

Graf, therefore, endorses R.L. Cleveland's accusation of archaeological fraud leveled against Mrs. Bent. It is unfortunate that Graf does not say whether or not he studied the case of the Bethel stamp and/or ever read Mrs. Bent's booklet upon which Cleveland bases his whole scheme. For my rebuttal of Cleveland's paper, cf. below, VII.

(5) The palaeography of the third text cannot be compared even remotely with any southern or northern Min inscriptions. The form of f, the only letter of unusual appearance, is unquestionably related to the Ḥaḍr f of Sumhurām; cf.

f of [q]yf of Ja 2885/1: (cf. JaMAR, IX, p. 99 and photo in pl. k = AIAAE, p. 45 and pl. 37);

of ḥyfn of Ja 2885/1: ; the upper part of the letter is unfortunately destroyed; and

of ʾlfdm of Khor Rori 7a: (cf. JaMAR, l.c., pp. 90-91, and photo in The Journal of Oman Studies, 1 [1975], p. 92, pl. 4a).

(6) The author's lack of respect for the texts is manifested by his invention of no less than three inexistent words for the sole reason of giving some meaning to his translations.

In his no. 1: he invents bn son of, which he puts between parentheses in his translation of l. 1; and in his third hypothesis of translation, he invents in l. 2 the temporal conjunction "when" which he does not even put between parentheses.

In his no. 2/4: he invents the inexistent interjection, "O," which he does not even put between parentheses.

B = JaT 99 = GrDM 1 (pp. 555B and 558; facsimile on p. 557, and pl. 121B).

Measurements of the stone: about 50 x 15 cm.

Leonard's facsimile — My tracing — 0 5cm — JaT 99

The author reads the text as follows (p. 555B):

"[ʾ] *bgr wkn'l* 'Abgar (son of) Wakan'il
byt blwh dl wasm built his tomb, Dall."

1 - The elongated trapezoidal cartouche with the two long sides slightly concave is not acknowledged by the author.

2 - The four dividers, the first three and the last one being, respectively, short lines and a dot, although simply mentioned as "word dividers" in the introduction (p. 555A), found no place in the transcription of the text. In either place, the author does not say how many word dividers there are; where they are found; and what shape each has. For the word dividers in Thaa, cf. JaMAR, XV, pp. 154-156.

3 - Five unexplained inconsistencies are immediately visible.

a - The initial "[ʾ]," which is supposed to be restored (cf. the two brackets), becomes certain in the translation.

b - The author still indulges in an old error already denounced 21 years ago in JaTS (p. 2), and repeated 16 years ago (cf. JAOS, 92 [1972], p. 521B), viz. the gratuitous restoration of the inexistent bn son of, which is doubtful in the translation.

c - The "w" becomes "W."

d - The inaccurate "d" of the transliteration becomes "D" of the translation.

e - The existence of the wasm is not accounted for in the translation.

4 - The author's reading of the text must be corrected in four places.

a - The tiny bar retained by the author as the left vertical section of his "[ʾ]" is much narrower and shallower than any section of any letter, and thus must be discarded.

b - The seventh letter of l. 1 reads n, not "n" of his "wkn'l."

c – The fourth letter of l. 2 is k, not "b" of his "blwh." In both these two instances, the author eliminates the upper section of the letter.

d – The fifth letter of l. 2 is n, not "l" of his "blwh." Here, the author eliminates the lower appendix of the letter.

5 – The author justifies his rejection of w of his "wkn²l" as a conjunction by saying (p. 555B) that "another individual would present problems with the singular pronoun that follows."

He does not spell out either the nature or the number of his so-called "problems" (in the plural); and it is difficult to figure out what they might be. His reason is nonetheless unacceptable because his interpretation of his "blwh" remains gratuitous as it could easily be a personal name. Furthermore, the author had forgotten all about his remark when writing the grand finale of his commentary, the THIRD hypothesis (cf. below).

6 – The author's main argument is this (pp. 555B and 558A).

[A] "The verb *byt* (Ar. *bata*) appears in Minaean (WR Min. 10) and Safaitic (CIS V, 28 and SIJ 139), and in the contracted [B] form *bt* in Thamudic (BIT, p. 512). In these contexts, it is usually rendered "to spend the night" or "to pitch a tent," but [C] here must have the meaning of "to make, build" (cf. LP 595 with comments), as it is followed by *blwh* "his tomb"."

Ad [A] – There are, of course, more than two Saf texts mentioning byt; cf. also, e.g., CIS 82, 230, 310, etc. Furthermore, the contracted form bt also exists in Saf (e.g., CIS 415, 1332, 1460, etc). As for Tham, it suffices for the author not to find the verb byt in vdhrIT (p. 511B) who refers only to his p. 512A: "bt, passer la nuit," and his knowledge of Tham does not go beyond. The form byt also exists in Tham; e.g., Eut 503/3, 529, JaT 36c, etc.

Ad [B] – The translation of b(y)t as "to spend the night at" is self-evident; cf. also in Liḥ, cf. CalL, p. 127, no. 99. But, LiSI's referred-to commentary(p. 154) on LP 595, viz. the verb byt "may also mean 'he made a bait', i.e.'he pitched a tent'," is erroneous because the verb does not have any direct complement. The idea of byt, 2nd form, meaning "to put up (a tent), to build (an habitation)" belongs to classical Ar (cf. DAFA, II, p. 934); it does not belong to NA; cf., e.g., "faire passer la nuit à (qqn)" (cf. A.Barthélemy, Dictionnaire arabe-français. Dialectes de Syrie, Alep, Damas, Liban, Jérusalem, Paris, I[1935], p. 71), "loger, cantonner, mettre en garnison (les soldats)" (cf. C. Denizeau, Dictionnaire des parlers arabes de Syrie, Liban et Palestine, Paris, 1960, p.55), and "faire passer la nuit, faire coucher//Donner à coucher" (cf. M. Beaussier, Dictionnaire pratique arabe-français (arabe parlé en Algérie et Tunisie), ..., 1958, p. 92A)

7 – Then, the author refers exclusively to "the rare Safaitic bly, 'tomb' in WH 163, 165 and 166" (p. 558A).

But, Saf bly is a personal name in WH 495;

the verb to shelter in WH 410; and

the noun shelter in WH 163, 165 and 166.

8 – The author adds (p. 558A) that

"The noun *byt* is well attested in (Nabataean and Palmyrene) expression funerary contexts; see the Late Aramaic *byt ʿlm* "grave"."

The author's expression "funerary contexts" is misleading because there is nothing "funerary" about byt; the idea of "grave" comes from ʿôlam eternity. Similarly, b(y)t means temple instead of house depending on the context (e.g.,in Liḥ JaL 102/6) or on the qualificative of b(y)t (e.g., the Sab byt/²laqh or Ar al-baytu l-ḥarâm). As a matter of fact, the noun b(y)t is much more often found in "religious contexts" than in "funerary contexts."

9 – The author further notes (p. 558A) that

"The absence of any of the familiar *khf, gbr, or mubr)* also reflects the Lihyanite words for "tomb" (e.g., *kfr,* peculiarity of the language in this graffito."

But, the idea of "tomb" exists only in his preconceived, gratuitous understanding of the text, and nowhere else. Furthermore, khf means funeral grotto; "gbr" is an error instead of qbr (the same error, "g" instead of q, is found on p. 564B: "Qedar (gdr)"); and "mtbr" also is a mistake instead of mtbr funeral chamber. The author's statement proves not only that the idea of "tomb" comes from his preconception, not from the text itself, but also, and most importantly, that the author erroneously deduces from the absence of a Liḥ word in the text to the "peculiarity" of the language of that inscription. Of course, each language has its own "peculiarity." But, the author's knowledge is so embryonic that he does not even realize that the whole Liḥ vocabulary is not yet known and, therefore, that a new Liḥ word for "tomb" cannot be excluded.

10 – The author continues saying (p. 558A) that

"It should be observed that personal restricted to South Arabic and Safaitic," names beginning with *Ḏl* appear to be

The reader does not expect the author to be aware of the following Tham personal names, viz. ḏll in Eut 47a, ḏln in Eu 247, and ḏlt in JaT 34a/1.

11 – The author adds (p. 558A) that

"*Ḏāll* ... appears as a personal name in Safaitic (CIS V. 3066)."

The name ḏl also exists elsewhere in Saf, e.g., LP 1901a, but it is also a verb, e.g., WH 3597a. Note also the difference in the spelling of "Ḏāll" here, but "Ḏall" in his translation (cf. above, p. 115).
Graf's references under nos. 7 and 11 (cf. the preceding page) were copied from WiHaI, p. 631, and HaAIC, p. 384, respectively.

12 – The grand finale of the author's commentary proves that he really does not know what to do with the inscription. Beside his translation reproduced above (p. 115), he suggests no less than three different hypothetical interpretations (p. 558A), which have in common that l. 1 has NO grammatical connection with l. 2, viz. l. 1 would contain two isolated names without any verb and/or any preposition to introduce the first. Even at first sight, such an understanding is condemned by the whole Tham material.

a – The first hypothetical alternative.

Pretexting that "Ḏāll" exists in Saf as personal name, he declares (p. 558A) that "Here it may be the name of the individual who constructed the tomb."
This hypothesis adds another irrealistic feature, viz. an isolated line composed of a verb followed by its direct complement + h which, in turn, is followed by the subject of the verb; and the pronoun -h would apparently refer to either of the two personal names of l. 1, which is not connected grammatically with l. 2. His translation would, then, be " 1 ʾAbgar. Wakanʾil.
2 Has built his tomb Ḏāll."

b – The second hypothetical alternative.

Taking into consideration A.F.L. Beeston's suggestion of ḏl as the name of the tomb, the author comes up with the following translation of l.2 (p. 558A): " "The house of his monument is (called) Ḏāll"."
Here again, who is the subject of "his"? Furthermore, such a translation implies a redundancy, viz. "the house of the monument" which is made possible thanks to the quiet switch of "tomb" to "monument" as the meaning of "blw." But, that is Beeston's way of argumenting! Finally, note that the author maintains his second vocalization "Ḏāll."

c – The third hypothetical alternative.

In order to make it look very scientific, Müller complicates things as much as he can. This alternative is introduced by the remark reproduced above, p. 117, under no. 10. If the author wishes to suggest that ḏl has to be a verb, as if such a feature were unknown, he is also wrong because Tham offers the verb ḏl in JaT 33c, and ḏll in JaaT 741; cf. also the 4th form hḏll in Nu 251/1. In fact, the author's remark introduces the hypotheses of W.W. Müller who apparently does not know what to do with "blwh" which he understands as

either b + the place name lwh;
or b + the verb lw + -h, the verb "lw" "being associated with the Aramaic verb lwa, lwi, lawwi ('to accompany,' or 'to join'), i.e., as b - lw - h, 'in his company'" (p. 558A). In fact, this verb is Syriac. This is another good example of the gratuitous and hasty concoctions characterizing Müller's work: he does not justify the loss of the third radical: Syriac lwʾ or lwy = here lw. Since the verb corresponds to Hebrew lwh, why did Müller not consider lwh as the whole verb instead of dissecting it into lw + h? Furthermore, why did Müller not retain what should have been appealing to him, viz. the meaning of "burial, funeral" which is indicated by several words of the same root? Moreover, Müller does not even mention, even less justify, his gratuitous dissection of "blwh" into two or three sections. The only justification possible would have been to prove that the word could not be understood as it stands, and this is impossible to prove. Besides, to justify his expression byt b-, Müller should have referred to that expression in Saf, e.g., CIS 310 and WH 641a. But, how could he know that since Saf is not his speciality? Finally, as it happened so often in the past, authors isolate an expression or a pericope and translate it according to their fancy without even considering the effect of their speculation on the whole text. Here again, Müller does not even attemt to suggest any translation of the whole inscription, although it seems certain that he endorses the normal meaning of byt. The author's translation based on Müller's second hypothesis is as follows (p. 558A):

" "Abgar and Kann(a)'il. He (Abgar?) has spent the night in his (Kanna'il?) company (or cave) when he was lost (understanding ḏl as a verb)." "

(1) Contrary to all his previous suggestions (cf. above, in particular, p. 116, no. 5), the author dissects his "wknʾl" into w + knʾl without giving any reason: why could the personal pronoun -h not refer to the "Wakanʾil" of his translation (cf. above, p. 115)? There is no justification to read "knʾl" rather than "wknʾl." Furthermore, which vocalization of "knʾl" is the reader supposed to retain, either "Kann(a)ʾil" or "Kannaʾil"?

(2) Whether it is "blw" or simply "lw" is immaterial for the author; what is important to him is to by hook or by crook inject into the text the idea of "cave" which becomes synonymal of either "tomb" or "monument" in the pro-

cess. But, such are the imaginative acrobatics imposed upon the inscriptions by incompetent writers.

(3) It would almost be natural to consider byt as an asyndetic relative proposition related to the second name, viz. just the opposite of the author's translation.

d – Here again (cf. above, the case of bn son of invented between the two names of l. 1), the author invents, without as much as to put it between parentheses and to mention it to his reader, the inexistent conjunction "when."

In fact, the contents of JaT 99 are as simple as can be, and there is no need to look for difficulties where there are non and/or to speculate for the sake of showing of. The text may be read as follows:

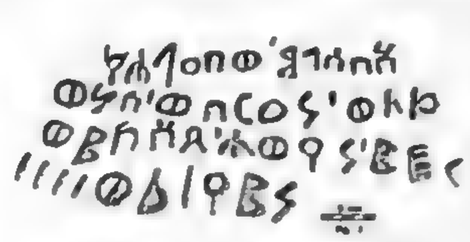
← ⌜hbgr'wkš'l' 1 Habgar and Kāš'il

← byt'knwh'ḏl Wasm 2 has given hospitality to Kinwuh [who] was lost. Wasm.

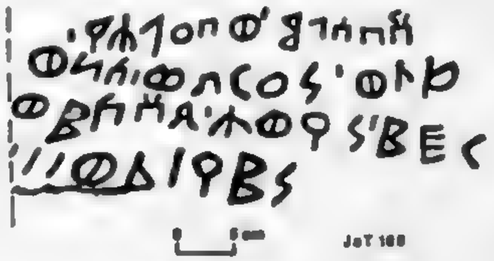
The two names of l. 1 refer to the husband and his wife and, as it so often happens in SA, the verb accords with the first and main subject, the one in authority. – hbgr, cf., e.g., Saf ʾbgr (e.g., Tap 29), and bgrt (e.g., CIS 4037). – kš'l, a qtll (l = il) nominal derivation; cf. the Tham name kš' in Hu 789a and Ḫā'il 203d, which reads as follows: bd mṭṭ wdd kš' Badd has insulted the lover of Kāši'. bd, cf. Saf CIS 4058. mṭṭ, cf. Ar maṭṭa, 2nd form "to insult" (cf. SaAAL, p. 985A). wdd, cf. JaMAR, XV, pp. 147-149. — knwh, cf. Saf knwt in CIS 2315. — ḏl, cf. the 2nd form "to lead someone astray" in JaT 33c, and the form ḏll in JsaT 761.

C – JaT 100 = GrGM 2 (pp. 558B, 560A-B, facsimile on p. 559, and photo in pl. 122A)

Measurements of the stone: about 42.2 x 25.5 cm.



Leonard's facsimile.



My tracing.

The author's reading of the text (p. 558B) is as follows:

" 'bslm wbrlḥy 'Absalim and Barlhayy
wnk wbrn whd and Nāk and Barān and Hadd
wmk'ṣ ḥwyn mṭr and Muka'is. Show us rain.
//// wd ymn O Wadd! Good fortune."

1 – Here again (cf. already above, p. 115), no trace in either the transliteration or the commentary of the seven word dividers, either large dots or very short bar in the upper half of the letter height or even a long vertical bar after wh (l. 4). The diacritical sign of both ḫ (l. 1) and ṣ (l. 3) is eliminated in the translation.

2 – The beginning of Graf's commentary sets the tone of confusion found in the whole of the author's prose. He states (p. 558) that

"The inscription begins in boustrophedon fashion, but the third line is retrograde, destroying the pattern."

The boustrophedonic ductus indicates only that the second line is engraved in the direction opposite to that of the first, but does not specify the direction of the latter. The expression "the third line is retrograde" makes no sense because "retrograde" means contrary to the previous direction; l. 3 precisely has the same direction as l.2.

3 – The author refers ʾbslm to Liḫ "JS nos. 21 and 144." First, correct "21" to 121/2; for some remarks on the text, cf. JaMIL, pp. 2, 6, 50 and 78. The second text is Min (cf. JaMIL, pp. 3, 6, 7, 11, 14-16, 19, 35 and 78). The same ʾbslm is also known in Liḫ JaL 145d, etc. The author also refers the name to "Thamudic (MNM no. 1)" (p. 558B), but this text is Saf.

4 – bʿlḫy is interpreted by the author as "The Lord lives" (p. 558B), which should be corrected to "Lord lives." Such an interpretation amounts to a truism. The name is a clear case where ḫy should be understood as a second form; thus, "Baʿal gives life."

5 – nk is referred to "Safaitic (SIJ 771)." It is also attested, e.g., in Ox 454.

6 – brʿn: the author states that the name is "probably derived from Ar. baraʿa, 'to surpass'" (p. 558B). His "probably" is absolutely unjustified, and instead of referring to the Ar root, why does he not refer to HaAIC, p. 102: brʿ in Saf. Indeed, the name is found in Saf; e.g., CIS 2515.

7 – hd is claimed to be "known from Safaitic (16 times) and Thamudic (only in TIJ 351)" (p. 558B). The number "16" is an error; HaAIC (p. 610) refers to "10" only. But, three numbers, CIS 4152, LP 811 and 1001 must be deleted, and ClAS 828 must be added. The initial name of ClAS 828 is khln Kahilām

(e.g., CIS 770) instead of the edition's "hsln." The Tham reference is erroneous because the name of HaLi 351 is hdd, not "hd," as here and in Hu 131.

8 – The author's etymological treatment of mk³ṣ (p. 558B) is bewildering. Nobody in his right mind would relate mk³ṣ to "mk³," as the author does. But, after that, he states that mk³ṣ "could be derived from Ar. Ka³aṣa." The author's "could" proves that he does not know what he is doing. Moreover, the author adds a suprising remark, viz. the name "could be ... an otherwise unattested byform k³ṣ; cf. kiṣ, 'to be greedy'." His "kiṣ" does not exist; what exists is kāṣa (i). Moreover, his vocalization of mk³ṣ as "Muka³is" is taken from from the 3rd form of Ar ka³aṣa, but such a form does not exist. Thus, mak³ūṣ, the passive participle of the 1st form.

9 – The author's interpretation of the last four words is imposed upon him by his own incredibly simplistic approach to the text, viz. his understanding of wd as "Wadd, the South Arabian Moon-God" (p. 558B; cf. below). It is a well-known fact that wd/wdd is used all through NA and SA as both the verb to love and the noun love or lover (cf. very recently, JaMAR, XV, pp. 147-149). wd as the Min lunar deity is not found once either in Saf or in Tham. It is known as such in one Lih text, JaL 77/2-3 (cf. JaMAR, VII, pp. 69-70 and tracing in pl. 18): ³fkl/wd the priest of Wadd, a manifest consequence of the Min influence, and Graf does not even refer to that expression. In that text, however, the context does not leave any doubt as to the interpretation of wd. In the present text, on the contrary, there is no clue at all even remotely suggesting Graf's understanding.

The author refers to four theophoric names with wd.

a – "³rswd (BID, which appears to be in Dedanite)" (p. 560A) is the author's first example. The author considers "w" as absolutely certain, but JaSaMA's copy (pl. 130: no. 127/1) clearly shows a square ʿ. The text is Lih and does not contain any mention of wd, as shown in JaMIL, pp. 20-21.

b – The author's second example is "³grwd (RES 3371 = M335, a Minaean graffito from Dedan)" (p. 560A). The name does not exist; it is ³gr wd = ³gr/wd (RES 3805/4) Wadd's hireling, viz. a worker at the service of the Min lunar god.

c and d – "zdwd and ʿbdwd in Lihyanite (CIL 9.1 and 4)" (p. 560A). The text is JsaL 49 = JaL 77 (cf. above). The author's references are given in the reverse order, viz. l. 1 concerns ʿbdwd, and ll. 4-5 (not "4") zdwd. But, as already noted above, the text is naturally explained by Dedan being a Min colony: it is engraved by a native Lih who became a "priest of Wadd;" he bore a theophoric name with wd, viz. ʿbdwd, and gave another theophoric name with wd, viz. zdwd, to his own son. The features of the text have nothing whatsoever to do with a Tham inscription found hundreds of klm away.

Still unsatisfied with his interpretation of wd, Graf states another of his preconceived ideas as follows (p. 558B):

"What these six individuals express appears to be a petition to Wadd, the South Arabian Moon-god."

The use of the verb "appears" is erroneous: the only thing that "appears" is the author's determination to utilize and, by the same token (in his mind) to justify, his preconceived idea that wd is the Minaean lunar god.

10 – The author's preconceived ideas developed above determine his commentary. The author deals first with hwyn and refers it to "Aramaic hwy and Syriac hawwi, 'to show'" (p. 558B); thus, hwyn is understood as hwy + n. However, that verb, Syriac hw³, pael hwwy = hwh of Aramaic of the Achaemenid times (cf. HoAAH, Part I/2, p. 10B; F. Rosenthal) = hwy in Biblical Aramaic (cf. l.c., p. 24B; H.L. Ginsberg) and in Jewish Palestinian Aramaic (cf. l.c., p. 60A; E. Y. Kutscher) = hwh in Palestinian Aramaic texts (cf. J.A. Fitzmyer and D.H.Harrington, A Manual of Palestinian Aramaic Texts, Rome, 1978, p. 319A) and in Hebrew hwh is related to Ar wahā (i) (cf. BuWC, p. 217A, W. Baumgartner, Hebräisches ..., I [1967], p. 283B, and R.P. Smith, Thesaurus Syriacus, New York, 1981, col. 1208). As usual, A.F.L. Beeston had to speak his mind: he suggests that "there may be metathesis of the w and y and that the verb hwyn is derived from Ar. hyy, 'to live'" (p. 558B). The only justification possible of Beeston's concoction is to give some support to preconceived ideas which are unjustifiable in the text itself.

The impressive conclusion drawn by the author is (p. 556B) that "If -n is an object pronoun, the 3rd person pronouns in the North Arabian inscription provides one of the few non- corpus."

Unfortunately, the author does not even submit a single example of those "few ... examples" or, at the very least, specify which NA language he has in mind. In fact, there is none.

11 – After his and Beeston's remarks on hwyn, Graf brings up the most basic question of them all, viz. the decipherment of the text, the question which should have been the very first for the author to discuss. He states (p. 558B) that " As for wd, the d is not certain (cf. the d in hd in line 2)."

As a matter of fact, his reading of "d" is erroneous, as he himself should have deduced from the comparison with d of hd because the only possible reading of the

letter form in HIS facsimile is y. But, his copy, which is materialized in the facsimile, shows a willful distortion of the original: of an almost horizontal feature of the stone which runs below the two letters and at least two of the four preceding vertical bars, he retains just enough to close the space between the two lower extremities of the second letter; thus, he eliminates the most important part of what can be seen on the photo. The letter is h, not at all "d" or "y."

12 – The author states (p. 560A) that

"The word *ymn* may be derived from Ar. *yumn*, "prosperity, good fortune, good luck" (Lane VIII, 3064)."

The first two meanings of Ar yamana and the first of Ar yamina imply the idea of the right side of things (cf. FrLAL, IV, p. 523B), a fact which is confirmed by the western Semitic languages (cf. C.-F. Jean et J. Hoftijzers, Dictionnaire des inscriptions sémitiques de l'ouest, Leiden, 1965, p. 109, and R.P. Smith, Thesaurus..., col. 1605), Hebrew (cf. BuWG, p. 303A), Palestinian Aramaic (cf. J.A.Fitzmyer and D.H. Harrington, A Manual ..., p. 322A: "ymyn (right)"), Palmyrene-Hatran-Nabataen (cf. RoAAH, p. 46A (H. Ingholt): "ymyn right (hand), south"), and Jewish Palestinian Aramaic (cf. l.c., p. 62B (Y. Kutscher): "ymyn right, right hand"). The preceding information contradicts the author's affirmation according to which (p. 560A; my underlining)

"derivatives of the root in Arabic, as frequently elsewhere in the Semitic languages, have the sense of "right.""

13 – In an apparent attempt to justify his interpretation of the text as containing a prayer to the deity Wadd, Graf refers to several Tham texts and one Liḥ expression.

a – He states (p. 560A) that

"In pre-Islamic Arabian texts the expression normally used for "good fortune" in connection with prayers to dieties is *sʿd*."

This statement is erroneous: in Liḥ, Saf and Tham, sʿd means "help; felicity" or "to be happy" (1st verbal form) and "to help, favor" (the 3rd verbal form).

He gives as examples (p. 560A):

"see especially the ca. sixth/fifth centuries B.C. Najdi Thamudic petitions to Nahy (BIT = Hu 207 and 271) and ʿAttarsam (Winnett and Reed, 1973: nos. 137, 160, 161, and 203 ff)."

For Hu 207, where sʿdt is a personal name, cf. JaMAR, V, p. 61.

For Hu "271:" the reference is erroneous; the text only has three signs.

For Ḥâʾil 137, where there is no "sʿd," cf. JaMAR, XIII, pp. 168-169.

For Ḥâʾil 160: the text may be read as follows: hʿt[r] sʿd nṭlyt O ʿAta[r]samm! Naṭalīyat was happy. – hʿt[r]sm, e.g., Hu 465. – sʿd, 1st form, as in Eut 4046. – nṭlyt, cf. Ar naṭala "expressit vinum" (cf. FrLAL, IV, p. 297A); same nominal derivation as ḥṭmyt of JaT 49. HaAIC (p. 940A) lists among his "doubtful readings" "nṭl TIJ 402. Or ḥl?." But, there is no cross-reference to "nṭl" in "nḥl" (p. 583). This reading is excluded under "ḥl" (p. 197) where there is no Tham reference to the name. Under nḥl (p. 583), no cross-reference to "nṭl." For the text, HaLi 402, where the name is nḥl Nâḥil, cf. JaMAR, VII, p. 122, note 20. For Hâʾil 161: the text may be read as follows: hʿtrsm sʿd ʿl wdd ysnm qd O ʿAtarsamm! ʿUll was happy: he was in love with Yaslim [who] was leading. – ʿl, e.g, Eut 30. – yslm, e.g., Madaba 297B. – qd, Hu 399+398/2 (cf. JaMAR, VII, p.132, note 72).

For Ḥâʾil "203ff:" this reference does not make any sense as Ḥâʾil 203 is followed only by two numbers, viz. 204 which is a Palmyrene text, and 205 which has only a few letters of the Ar alphabet. Ḥâʾil 203 is divided by F.V. Winnett into 6 groups, a, b, c, d/1-2, e/1-5 and f; and in his edition, the only line with nhy and sʿd is Winnett's "e/1" = my f; for this text, cf. JaMAR, XV, p. 179.

In fact, the author's argumentation perpetuates the preposterous approach standardized by A. van den Branden and especially by F.V. Winnett according to whom a text beginning with an invocation to a deity necessarily contains a prayer to that deity. Such an approach is one of the ingredients of Winnett's last paper in JAOS (117 [1987], pp. 239-244) which is thoroughly rebuked in JaMAR, XV, pp. 124-191.

b – The author then adds (p. 560A) that

"In Lihyanite, the standard blessing formula is *frḍyhm wʾḥrthm wsʿdhm*, "(for) their successes, and their posterity, and their good fortune" (Caskel, 1953: 76)."

The reader notes that, on p. 560A, Graf uses two different sigla to refer to the same publication, CaLL, viz. "CLL" and "Caskel, 1953." The author is so ignorant that he does not even know the five following facts, viz.

(1) the difference between a preposition and a conjunction; f is the well-known imprecative conjunction (e.g., JaL 16a/1). And why does he put his "(for) between parentheses?;

(2) the difference between singular and plural: there is not the slightest indication that rḍy, even were it a substantive, might be a plural;

(3) the preposition f is a local preposition, not an attributive one;

(4) the preposition "f" has not yet been found in Liḥ.

(a) CaLL claims that the preposition

" ft aus dem „Thema" entlehnt, in (auch in übertragenem Sinne):
No 41, 34.1. Defektiv geschrieben."

((1)) His no. 41 = JsaT 251b (p. 57) reads as follows:

" حن كح فنر

„'Ammān hat einen Gegner im Kampfe überwunden."

((a)) First, two corrections. In CaLL, p. 25, l. 8, this text is referred to as "51" instead of "41;" and on p. 137A, "'lw" is erroneously attributed to the present text.

((b)) Caskel refers "kh" to Ar "kāb I einen Feind überwinden" (p. 138B). This is unjustified: the verb means "to overcome" and may also apply to humans as well as to things, such, e.g., as calamities, passions, etc. On the one hand, since the verb is not followed by any direct complement, it could only be translated as a passive, viz. the author "was overcome." But, such a translation is unacceptable because no writer is likely to make such a statement. On the other hand, it is most unlikely that the text was engraved by the man who overcame the author of the text because he would no doubt have mentioned his own name. Therefore, the verb kh = kwh is not appropriate for the text.

((c)) H. Grimme's interpretation of the text (cf. Le Muséon, 50 [1937], p. 108) must even at first sight be discarded because of his misreading of 3 (out of 8!) letters.

((d)) The text may be read as follows:

'm nkh fdr 'Amm got married and was harmed. – 'm, JaL 13. – nkh, cf. Ar nakaha "to marry," wed" (cf. SaAAL, p. 1104A). – f, a coordinative conjunction, as in Saf (cf. JaMAR, XV, p. 129), e.g., JaL 59B/2. – dr: since there is no clue of the circumstances during which the text was engraved, it does not seem necessary to interpret dr as drr to war; cf. Ar dāra (i) "to harm" (cf. LaAEL, p. 1812A); here, in the passive form.

((2)) His no. 56 = JsaL 345a (p. 102):

" 1 فشكرت 2 وداد بلست 3 يدع لن ط

„Ein Gruß von Balamat an Sākirat. Ich bin Yādi'."

The author's translation arranged according to the word order of the original reads as follows: "an Sākirat ein Gruss von Balamat. Yādi' bin ich." Even at first sight, such a phraseology is most unusual. It can be seen the kind of meaning Caskel gives to his "in übertragenem Sinne:" the locative preposition "in" becomes ... attributive, "for, to." The preposition f does not exist in the text; cf. JaMAR, VII, p. 123, note 26.

((b)) WiReAR (p. 175A) refers "1. prepos. 'for (to, for)'" to his no. 6. This text is composed of JsaL 79 and 80, and JaMAR's documentation on the texts (pl. 85 and 110) is far superior to Winnett's. There is no "f" in the texts; cf. JaMAR, VII, p. 128, note 48.

(5) Finally, Graf does not know that the forms rgy, 'hrt and s'd are verbs (not substantives, as in his translation) having the deity as subject, as already demonstrated by A. van den Branden almost 30 years ago (cf. al-Machriq, 50 [1960], p. 100), followed by JaMIL (p. 134) and WiReAR (p. 125, no. 9). The latter, however, failed to recognize that he republishes the same text as A. van den Branden, viz. JaL 144.

c = Finally, the author endorses WiReAR's interpretation (p. 115) of JsaL 138, which is based on A.F.L. Beeston's elucubrations and is rebuked in JaMAR, VII, p. 116, note n. Obviously enough, Graf ignores this publication. To the seven objections listed there, I wish to explicit three others.

(1) By refusing even to mention the possibility of n'rgd as a personal name, an hypothesis confirmed by the parallel name glmgd (JsaT 314/2), WiReAR proves that an epigraphical fact has no value when compared with his preconception on the use of the name Gadd.

(2) Verbs, such as "to moisten" (cf. Ar tāra [i], 2nd form) and "to favor" (cf. Ar na'ama, 4th form) are transitive and do not need any preposition to introduce their complement. Yet, the text clearly shows the preposition b introducing the personal pronoun -h.

(3) The engraver's use of the word dividers excludes WiReAR's dissection of n'rgd into "n'r gd." A word divider is placed after six words, viz. khf, kbr'l, mt'l, ddn, trv, n'm, although it is UNnecessary in the first five cases; it was necessary only between trv and n'm. The word divider is omitted only in two cases, viz. after bn and mlk because it is most UNnecessary there. By supposing that n'r and gd should be separated from each other, Winnett puts n'r on the same level of intelligibility as bn and mlk. But, while the last two words are w e l l attested, "n'r" would be an hapax legomenon.

JaMIL (p. 42) publishes my translation of wtrv/n'm/bh/n'rgd as And richess and pleasure [have been] with him. Na'argadd. It seems in order to mention here again that H. von Wissmann considered the above-mentioned translation as "einleuchtend" (cf. Paulys Realencyclopädie der klassichen Altertumswissenschaft, Supplementband XII, Stuttgart, [1970], col. 949). I wish, however, to revise my translation of the text because the conjunction w is inexistent before n'm; my new wording is

as follows: And pleasing richness [was] with him. Na^{c}argadd. Na^{c}argad is the name of the engraver, as already suggested by JaSaMA, (p. 470) as a possibility.

14 – Nowhere in his commentary does Graf even mention the four aslant bars at the beginning of l. 4. The initial bar and the lower half of the second (from the left) are cut off by his photo. It is difficult to figure out exactly what those bars may be connected with or stand for. As far as the text is concerned – and we have nothing else to go on – plurality is clearly visible in the personal names, viz. six of them, which may well represent a whole family. Then,

l. 1 would list the husband ^{3}bslm, and his wife b^{c}lhy;

l. 2 would give the names of their three sons nk, br^{c}n and hd; and

l. 3 would mention the last child, a daughter mk^{3}ḳ. Thus, 4 bars for 4 groups?

15 – The text JaT 100 may be read as follows:

^{3}bslm^{3}wb^{c}lhy	⟵ 1	^{3}Absalam and Ba^{c}alhayy
wnk^{3}wbr^{c}n^{3}whd	⟵ 2	and Nayk and Bar^{c}ân and Hadd
wmk^{3}ḳ^{3}ḥwyn^{3}mtr	⟵ 3	and Mak^{3}ûḳ, the dark ones, have hastened
[/]$_{1}$/$_{1}$//wh^{3}yman	⟵ 4	[/](/)// – Alas! – south.

ḥwyn, cf. ^{3}aḥwá (i), pl. ḥûw "dark, black" (cf. FrLAL, I, p. 447B, and LaAEL, p. 661B); cf. also the personal name ḥwy in both Tham VoEAI-T 5, and Saf, e.g., Ox 257. In Saf WH 1768 (cf. WiHaI, p. 281 and facsimile on p. 692; cf. my review of WiHaI in Orientalia, 48 [1979], pp. 478-528), the editors read differently as "sr" the last two letters although they are identical; the last word of the text reads wss He was rewarded. Cf. Ar wassa to reward (cf. SaAAL, p. 1201A). -n, plural ending, as in Saf; cf., e.g., ḥrbn in CIS 1607. – s^{3}r, cf. the Ar expression saṭara fî l-^{3}ardi "he (a man) went away in, or into, the country of land; hastened"(cf. LaAEL, p. 2721B). – wh, cf. Ar wayha, wayhi and wayh^{an} "Et dolorem aut commiserationem significat" (cf. FrLAL, IV, p. 514B) and "Woe! Alas! (cf. SaAAL, p. 1243B) . – ymn, e.g., Lih JaL 143/4, which is the republication of no. 7 of WiReAR, p. 124.

D – Ja 3203 = GrDM 3 (p. 560B, facsimile in p. 561, and photo in pl. 122B).

The text is a vertical name in an incomplete cartouche.

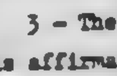
Leonard's facsimile: : my tracing.

The author reads the [illegible] as follows (p. 560B):

" *Tfynm (or Mnyft) Tifyānm (or Munayfiy).*"

1 – The cartouche, which is not even mentioned in the commentary, is open on the upper left and lower right corners.

2 – The author's initial statement,"There is no clear indication as to which direction this name should be read ," only proves his gross ignorance. He is so ignorant of epigraphy that he does not even know that, in the case of a vertical text as here, the normal reading of the letters is from top to bottom, and rarely from bottom to top; and his reading from bottom to top is contradicted by palaeography (cf. below).

3 – The authors adds that " the *m* and the *n* face in different directions." This affirmation is erroneous because a senestrograde (the normal) ductus ⟵ becomes  in a vertical line to be read from top to bottom; and this is exactly the upper part of the present rock inscription.

4 – The author continues in the same vein. He writes that "it seems more likely that the former is the reversed letter" , viz. the letter m, on the basis of some Min graffiti from Rams (cf. RB, 43 [1934], p. 590, "nos. 2-4"). But, he erroneously refers to no. 3, which is perfectly normal; and in the two other instances, the two letters m's are manifestly dextrograde in senestrograde names and, therefore, they are exceptions to the normal ductus.

5 – The author's conclusion to the effect " that therefore the inscription should be read from ground level to the top of the stone,"

is erroneous because there is no need or justification to seek difficulties where there are none. Furthermore, the author's choice of his two examples proves his lack of objectivity, viz. his determination to, cost what it may,prove his predetermined solution. He chose those two examples exclusively because they show two cases of m engraved in the direction opposite to that of the line. But, he sweeps under the rug the texts where it is the letter n which is the exception (e.g., Ja 2253d, 2258d, 2260f, etc.), and especially the texts where n is the exception and m is normal (e.g., JsaM 150; Ja 2254, 2338, etc.). Therefore, the value of his two examples is nil.

6 – The author's conclusion inferred from his distorted premises is that " The name can then be understood as frequently in Minaean or in imitation of *ʃfyn(m)* with the mimation found Minaean usage."

Beside being erroneous, his conclusion also contains the error of putting m but-

ween parentheses. – The author refers "tfyn" to "only" three Sab texts. The reference to Gl 1596/1 must be added. Graf copied his three Sab references from SabIC, p. 146. – Only an ignorant would speak of the mimation only in relation with Min. – He affirms that " For the vocalization
Ṯifyān, compare the name Sifyān (CIK II, 515)."
In the volume referred to, although the name is printed more than fifty times as "Sufyān" (pp. 515A-516B), the author still succeeded in miscopying it as "Sifyān." – The Ar vocalization is valueless here because it only shows how the Arabs pronounced names not belonging to their own culture. Moreover, the vocalization of "tfyns" as "Ṯifyāns" is inexplicable. – The author is not even sure of his own concoctions because he ends up mentioning that the name could be " derived from Ar. *ṯafan*," which should be corrected to tafana. He, however, fails to mention the nominal derivation he would apply to the name.

7 – Here again, the author is not even sure of all his preceding hypotheses because his commentary ends with two contradictory confessions, viz.

" It is also possible that the name is to be read from the top of the stone to the bottom and understood as *munyft* (derived from Ar. *nfṯ*"

and

"since this name is unattested elsewhere in the North or South pre-Islamic onomasticon, the reading of *ʃfynm* is more likely correct."

The second statement shows only his ignorance.

The name is to be read from top to bottom as mnfyt Munayfat. The name is Ḫaḍr (cf. above, p. 114 for the study of the letter f), and is the Ḫaḍr spelling of the other Ḫaḍr name mnyfs of Khor Rori 6b (cf. JaMAR, IX, p. 90).

VI – Note on J. Cermat, JaS 196, 197 and 198.

A – Introduction.

1 – In a short paper entitled "Ein safaitischer Schlüssel vom Ǧebel Qurma"(cf. ZDVP, 102 [1986], pp. 110-112 and pl. 9-11), U. Hübner and E. A. Knauf deal firstly with a small truncated stone cone (pl.9B and 11A) of 11 x 7.5/5.5 cm, weighing 750 gr and presented as a hammer, and secondly with three Saf inscribed stones in footnotes 5-7 (cf. pl. 10A, B and 11B). According to footnote 1 (p. 110), the two coauthors are each responsible for the wording of their respective section, viz. I (pp. 110-111) and II (pp. 111-112), but "beide Verfasser stehen inhaltlich hinter beiden Teilen." Therefore, they are both equally responsible for any part of the paper.

2 – The title of the paper unequivocally speaks of the object as being Saf. Yet, according to p. 111, the object is merely assumed to be more likely Saf on the basis of both the history of the jebel and its inscriptions. But, the main fact remains that it was found above ground and the paper does not refer to any type of excavation which might have yielded the object. Furthermore, from an historical viewpoint, the paper says only that the object is not neolithic. It is an unusual latius hoc to deduce from that negative information that the object is Saf. Finally, neither the inscriptions from J. Qarmat already published, WH 3901-3936 (cf. WiHaI, pp. 536-539) nor those edited by the two coauthors can prove the Saf origin of the object which could have been either lost or dropped by a passing-by traveler. Note that WH 3914 (cf. WiHaI, p. 537, and a small photo in p. 739A) was republished by M.C.A. Macdonald in ADAJ, 24 (1980), p. 197, and two larger photos in pp. 358-359; JaMAR, XIII, p. 234, has reedited the text. Note also that the name of the place is given as "Ǧebel Qurma" in the present paper (pp. 110, 111, and pl. 9-11), but as "Tell Qarma" by WiHaI, pp. 2 (map), 348 and 536. Gazetteer – Jordan (Washington, 1971, p. 282B) gives three names, viz. "Jebel Qarma," "Tell Qarma," "Jabal Qarmah," each of them referred to "Jibāl Qarma," but the spelling "Qurma" is not mentioned. The concordances of this jebel are 31°47'N-37°11'E. Furthermore, since the concordances of (al-)ʾAzraq aš-Šīšān are 31°50'N-36°49'N, according to the same Gazetteer – Jordan (pp. 30B and 67A), J. Qarmat is southeast of (al-)ʾAzraq, not "east" of that location, as given by WiHaI (p.536), " "Ǧetlich" of the present paper.

3 – The small end of the object is very well rounded while the opposite extremity is (p. 110) flattened and elliptical. The coauthors interpret the cone as a writing tool, a "Schreibgerät" (p. 111), on the basis of both the inscriptions and the petroglyths found in the vicinity. Here again, the mere presence of inscriptions and petroglyths cannot prove the coauthors' thesis, and not even a single drawing of the latter is shwon in the plates.

4 – The caption of pl. 9B states that there are traces of usage on the large elliptical side of the object. Such traces cannot by themselves prove the coauthors' thesis which remains gratuitous. Indeed, the coauthors do not even mention, less prove, that the object could not have been used either as a pestle or as a punch. Furthermore, that elliptical side does not appear in the photo to be more damaged or more used than the lateral circular side (cf. pl. 9B a and b). The coauthors do not even attempt to prove that the elongated hole visible in the left section of that circular side (cf. pl. 9Bb) is not, and could not be, original; and the same photo also shows large holes on that circular site.

5 – On the complex question of engraving tools, whe have to wait for a report from B. Bowen, "a professional stone-engraver," which is supposed to appear in "the final publication" of "The Corpus of the Inscriptions of the Jordan Project" (cf. M.C.A. Macdonald in ADAJ, 26 [1982], p. 171A). But, it is to illustrate some engraving types that the coauthors publish three Saf inscriptions, and claim (p. 111) that inscriptions, such as no. 3, can be directly produced with such a hammer. None of the three photos, however, is accompanied by a scale, and no measurements of the texts are given in their respective footnotes. It is, therefore, impossible, for the reader to figure out how wide the tracing of no. 3 really is, and the expression "kaum vertiefte, dafür relativ breitflächige Buchstaben" (p. 111) applies to too many texts and is incomplete and too vague to be of any help. Furthermore, unless the tracing is very wide, which does not seem to be the case, and the depth of the tracing diminishes toward each letter extremity, that tracing cannot be produced with such a hammer because of its well-rounded, elliptical edge.

B – JaS 196 = J. Qarmat 1a (cf. footnote 5 on p. 111, and pl. 10A).

The text may be read as follows:

l'qrb bn 'ds bn mlkt wtzr html w'zt — By 'Aqrab, son of 'Ādis, son of Malkat. And he was on the lookout for Hatamil and 'Azzat.

same decipherment as in the edition, except for the proper name. – l'grb, e.g., Tap 8. – 'ds, e.g., WH 3699 (cf. JaMAR, XIII, p. 252). The edition vocalizes the name as "'Udas (oder 'Addās)," presumably according to Ar onomastics. But, these cannot be imposed upon Saf texts which belong to a different culture. Furthermore, the qttl (reduplication of t) nominal derivation cannot be applied here because it is wellattested as such in Saf; cf., e.g., ʾlld (CIS 464), ʾllg (WH 1053 in JaMAR, XIII, p. 203), hrrd (WIAM Saf 4 in l.c., VII, p. 140, note 126), zrrb (WI 466 in JaSH, p. 82, note 128), etc. – mlkt, e.g., WH 2949 (cf. JaMAR, XIII, p. 226). The edition still indulges in considering mlkt as a qtylt nominal derivation, "Mulaikat," an opinion which was more than 15 years ago rejected as gratuitous (cf. Orientalia, 40 [1971], p. 280). That formation also is attested as such in Saf; cf., e.g., wlyht (CIS 1212; cf. AION, 31 [1971], p. 32), and hnymt (LP 691+692 in JaSH, p. 131); cf. also the qtyl formation in ʾsyd of JaS 198 (cf. below). – html instead of "h-my 'den Himmel'" of the edition. The name belongs to the qtll (l = il) derivation; cf. also, e.g., htm (e.g., ClAS 764 in JaMAR, XIII, p. 76) and hytm (ClAS 779 in l.c.). – 'zt, e.g., WI 986. As a consequence of its reading of the preceding word, the edition considers 'zt as a substantive and translates it as "den Abendstern ([ha'-]'uzzat)" [sic!!], thus inventing an inexistent article and confusing the name 'zt with that of the old deity al-'Uzzá ('zzy) (cf. T. Fahd, Le panthéon de l'Arabie centrale à la veille de l'hégire, Paris, 1968, pp. 163–183).

J. Qarmat 1b (cf. pl. 10A). – According to the edition, the existence of an inscription "darunter," viz. below text 1a, is doubtful (cf. end of footnote 5 in p. 111). No sign of letter can be seen below text 1a, but there is an inscription above it (darüber), which is not mentioned in the edition: l'b... By 'Ab... The balance of the text is unreadable in the published photo.

C – JaS 197 = J. Qarmat 2 (cf. footnote 6 in p. 111, and pl. 10B). The edition reads the short pedigree as "l-ʿry bn nrn 'Von Nūrī b. Nūrān'." Therefore, the edition misreads the large arc (second letter), r, as "n" which is a very short straight bar; the three small arcs, three b's, as "r," "b" and "r," respectively; the fifth letter, b, as "y," and eliminates the short straight bar (fourth letter), n. The text may be read as follows: lrbnh bn nbn — By Rabnuh, son of Nawbān. – rbnh, cf. rbn, e.g., in WH 1813 (cf. JaMAR, XIII, p. 213) and rbny in WH 3810. – nbn, e.g., nb in WH 2050 (cf., l.c., p. 217). – The short pedigree is immediately followed by hml

(as in the edition) the camel.

D = JaS 196 = J. Garmat 3 (cf. footnote on p. 111, and pl. 11B). The edition reads the text as "l-ʾrqd w-ʾśhl 'Von Arqad und Aśhal'." It is regrettable that the coauthors use the confusing transcription of śin as "ś" instead of the commonly accepted š. The same remark is also applicable to the Tham text CISN 2 (cf. footnote 10 on p. 112), which they consider as "südsafaitisch" instead of Tham; and this their classification is another source of confusion. In their bibliographical note, "(1978) [36" must be corrected to "(1976-1977)" and "[35," respectively. Furthermore, they speak of the first name which they read "tmdśr *Taim-Duśarā." The name is the second of the inscription; their reading cannot be justified by the facsimile; and the photos are not good enough to ascertain the definitive reading of the whole text (cf. JaMAR, VIII, pp. 15-16). Here again, the plurality of subjects in Saf inscriptions has already been rejected as inexistent more than fifteen years ago (cf. Orientalia, 40 [1971], p. 283). Here again, the edition misreads four letters, viz. the third, s, the fourth, y, the eight, b, and the tenth, r, respectively, as "r," "y," "ś" and "l." - The text may be read as follows: l'syd w'bhr By 'Usayd. And he became rich. - 'syd, a qtyl nominal derivation (cf. above, commentary on JaS 196); e.g., CIS 5136, and 'swd in CIS 3192 (cf. JaMAR, XIII, p. 192). - 'bhr, cf. Ar bahara, 4th form "to become rich, flourishing (again) (cf. DAFA, II, p. 872A).

VII - R.L. Cleveland on the clay stamp from Bethel and a vindication of Mrs. Theodore Bent.

A - Introduction.

D.F. Graf's endorsement (cf. above, p. 114) of R.L. Cleveland's paper on "More on the South Arabian Clay Stamp Found at Beitîn" (cf. BASOR, No. 209, February, 1973, pp. 33-36), was a painful reminder that no scholar had in the last fifteen years taken up the gauntlet to vindicate Mrs. Bent, and that some authors had even taken that paper for granted.

Cleveland's arguments appear even at first sight specious and exaggerated, and his allegations against Mrs. Bent are not only limited to mere concoctions, but also shocking and hard to believe as they are leveled against a lady who, having died on July 3, 1929 (cf. Who Was Who 1929-1940, London, 1941, p. 98A), cannot defend herself. Mrs. Bent's booklet, Anglo-Saxons from Palestine or the Imperial Mystery of the Lost Tribes (London, 1908), where Cleveland claims to find "new clues" (p. 34, l. 1), or "further clues" (p. 35, l. 19), or "additional clues to the mystery" (p. 35, l. 18) of the clay stamp, presents a text simple and easy to understand; yet, it is systematically misconstrued by Cleveland. A preconceived theory alone can hardly, in my opinion, explain such a systematic distortion of the written word; there has to be a deeper reason behind that theory, which would account for the steady application of the scheme not only upon the simple wording of Mrs. Bent's booklet but also upon the disregard of the testimony of James L. Kelso, the archaeologist who unearthed the Bethel stamp in 1957. This is the only true "mystery" in the matter because Cleveland's paper does not provide any clue as to the nature of that unspecified, unacknowledged reason behind the scene. Cf. also below, the Addendum in p. 326.

The study of Cleveland's paper and related documentation was not done by Y. Shiloh who very recently mentioned the case of the Bethel stamp in Eretz-Israel, 19 (1987), p. 292A. The author confines himself to giving a very succinct resume of Cleveland's thesis without apparently studying the archaeological evidence at hand and even reading Mrs. Bent's publications. Yet, he discards the Bethel stamp from his research on the sole basis of Cleveland's position.

B - Cleveland and the clay stamp.

1 – Cleveland introduces his discussion on the Bethel stamp with the following statement (p. 33, ll. 14-16):

"the original publishers of the piece, supported by its excavator, answered the challenge in a competent, but not entirely conclusive, rejoinder."

It is, thus, unnecessary to take up the objections of the challenger, Y. Yadin in Eretz-Israel, 10 (1971), pp. 146-149, pl. IX; and xiiiB of the same periodical states that "En[sic!] English version of this paper appears in" BASOR, No. 196, December, 1969, pp. 37-45. It should be noted that Cleveland never mentions the Eretz-Israel paper, even when referring to Yadin's challenge (p. 33, ll. 9-12, and footnote 3). Furthermore, Cleveland's preceding statement introduces the two sections of his paper, of which the first deals with five objections against the singularity of the Bethel stamp, while the second contains the author's thesis on Mrs. Bent's alledged fraud. It should be strongly emphasized that those two sections never deal even remotely with the archaeologist's testimony which is offhandedly swept under the rug; cf. below the end of this section. It is, thus, against the background of such manifest a disregard of a crucial information that Cleveland's thesis is developed. But, that presentation remains faithful and well attuned to the general background by adding more examples of discarded important information.

2 – Cleveland's debate of five "points arising from the previous discussion" (p. 34, ll. 1-2).

a – The FIRST section (p. 34, ll. 2-16) is about the authorship of BeSA in three parts.

(1) Cleveland first belabors the point that (p. 34, ll. 2-4)

"First, the discussants have erroneously named Theodore Bent as the author of *Southern Arabia*."

Cleveland is wrong if he suggests that "the discussants" had the intention of denying the well-known part taken by Mrs. Bent in the writing of the book; it certainly was an unintended lapsus calami. On the contrary, the reader wonders how Cleveland, after making his point, could fail to reproach the very same omission to Mrs. Bent herself. Indeed, at the outset of her booklet, she declares herself to be the s o l e author of Southern Arabia, viz.

"Mrs THEODORE BENT
Author of Southern Arabia, Soudan and Sahara, &c." (p. III).

Cleveland omits such an important information because it is a major argument against his own thesis. This omission proves not only that Cleveland's investigation is not as accurate and exhaustive as the author would like the reader to believe, but also that he shuts his eyes to important facts; the preceding piece of information is also vital in another question (cf. below, p. 156).

(2) Then, Cleveland asserts that Mrs. Bent "compiled" (l. 7) Mr. Bent's "manuscript" (l. 7) and "rough notes" (l. 8) and "her own (rather full it would seem) 'chronicle' of the journey" (ll. 8-9). These details introduce the reader to another facet of Cleveland's method. His whole sentence deals with the narrative writing of BeSA. Yet, footnote 7 (p. 34), which refers to" rough notes " "contains one reference only, viz." *Southern Arabia*, p. x." But, the expression "rough notes" belongs to an explicative note appended to, and concerning only, the "Bibliography." The note reads as follows in BeSA, p. x, ll. 15-18:

" BIBLIOGRAPHY

Ali Ibn al Hussain, El Masudi, Abu al Hasan, Diodoros, Marco Polo, Sir John Maundeville, Pliny, the *Periplus*, Strabo, Ebn Said, Ptolemy, and others; but, as many of these names have been copied by me from rough notes of my husband's, I cannot be certain about the editions."

(3) Cleveland's preceding statement is immediately followed by this unexpected affirmation (p. 34, ll. 9-10):

"One may not safely attribute the same expertness to all parts of the book or expect uniformity."

Something is missing in Cleveland's argumentation because this affirmation cannot be the conclusion of his preceding statement. And since he does not give any clue or hint as to the reason justifying his "not safely," it is difficult to avoid considering his statement as the subjective springboard or introduction to his following statements which read as follows (p. 34, ll. 12-16):

[A] "That she did not state " We bought it " in reference to three items [B] does not necessarily mean that the pieces were not acquired." She may [C] not have recognized the squeeze as belonging to the clay stamp, so the [D] handwritten note on it " was of no benefit to her—or the squeeze may have been sent to Glaser before the book was written."

These statements present a desultory argumentation evolving completely outside the facts.

Ad [A] – Good logic forbids the understanding of a silence as proving exactly the opposite. That Mrs. Bent did not say that her husband and/or herself did not buy those antiquities cannot be understood as saying that they did "acquire" them. Indeed, Mrs. Bent never stated that those pieces were theirs (cf. already BASOR, No. 199, October, 1970, p. 61). To give some probability to his reasoning,

Cleveland should have given an acceptable reason why Mrs. Bent would have deliberately kept secret the purchase of those pieces. Instead of proving the point, he limits himself to a mere statement. While the larger stones ended up in the British Museum in 1926, the clay stamp and the two other seals "are now unaccounted for," as acknowledged by Cleveland himself (p. 36, l. 19). The expressions used in BeSA are strikingly different in each case. The objects acquired by the Bents are listed, viz. "we bought" (p. 435, l. 2);

"we brought from Al Gran" (p. 436, l. 8);

"we bought" (p. 436, l. 21); and

"we obtained" (p. 24 and also p. 125, ll. 1-2).

But, the language used in the mention of the two small seals and the clay stamp is quite different indeed; here is the text (p. 436, ll. 27-29):

> "In the same illustration are also part of an earthenware stamp and the seal of *Yerahal, the younger of Shibebm*, with its golden setting, and a copper seal with *Sabota* on it."

Cleveland's understanding of the preceding information is expressed as follows (p. 34, ll. 10-12):

> "The wording of the notes to the photograph under discussion is not, for instance, to be pressed too far."

The author does not give any justification for his statement which is an unscientific way of eliminating the positive testimony of Mrs. Bent's text. Furthermore, the apparent connection Cleveland puts between Mrs. Bent's remarks on the orthograph of some names and the caption of the photo is devious because Mrs. Bent did not need her husband's "rough notes" to know what happened when the photo was taken; she was there herself. Objectivity demands the accurate analysis of the facts instead of pressing them "too far," as Cleveland does by practically reducing Mrs. Bent's witness account to worthless minutiae.

The phraseology used in the two cases is so different that it is impossible to understand how Cleveland could have written his statement which is contradicted by the text itself. The phraseology used in the case of the three small antiquities suggests that these pieces were shown to the Bents for photographic purpose. Almost every traveler to South-Arabia finds himself sooner or later in the possibility of photographing an antiquity which the owner would not sell or give away. In such a case, Mrs. Bent could not say that her husband and/or herself had "bought" or "brought" or "obtained" all the antiquities shown on their photo. How could Cleveland imagine that the Bents would have received or even bought the two seals, one "with its gold setting," and the other in "copper"? The natives also -

and rightly so - do appreciate beauty and especially when gold is involved, and they keep such objects for themselves. Furthermore, "now a comparison of Bent's handwriting and that of this note [on the squeeze] has convinced us ... that Bent is the author of the note on the squeeze" (cf. BASOR, No. 199, p. 60). Cf. also below, Cleveland's thesis.

Ad [B] - This statement is contradicted by the keen sense of observation displayed by Mrs. Bent all through BeSA. Furthermore, the Bents had seen only three small antiquities, each one having its own characteristic shape; and it is not very difficult to remember that much. To think that Mrs. Bent would not have recognized that the squeeze, which must have been made on the spot, was that of the triangular antiquity, is ridiculous because the Bents were constantly working together and not at all as separate units, as pointed out by North British Daily Mail, which speaks of (cf. Press Notices, p. xii, ll. 33-35)

> a "widow, who had been always with him in his wanderings in Africa, Arabia, and elsewhere."

It is, thus aberrant to even think that Mr. and Mrs. Bent were not a well-knit unit and that Mr. Bent would have done anything without his wife knowing it and being fully aware of it. Furthermore, the importance of Cleveland's remark is capital: he implicitely recognizes Bent as the author of the squeeze.

Ad [C] - What is the exact meaning of that remark in the overall argumentation?

In fact, however, the remark contradicts the most important feature upon which Cleveland repeatedly bases his whole theory, viz.

> "Given Mrs. Bent's anxiety during those years and her clinging to the memories of her husband;"
>
> "devoted to the memory of her late husband;" and
>
> "leaving some special possession associated with her late husband's achievements."

Now, all that dramatic scenario of a most devoted, of a most dutiful wife is shattered by one exception: one writing of Mr. Bent was "of no benefit" to the same wife! It is regrettable that Cleveland did not even bring such an extraordinary fact to his reader's attention.

Ad [D] - The preposterousness of this alternative proves that the preconception of Cleveland's theory is strictly imaginative and was not checked against the facts. It is ridiculous even to think that the small squeeze (13.6 x 11.1 cm; cf. BASOR, No. 163, October, 1961, p. 15) would have been sent to E. Glaser when the Bents were still in the field; they certainly had more urgent things to take care of than to dispatch (how?) a small squeeze to Glaser from

the hinterland of South-Arabia. Indeed, the sending of that small squeeze was certainly not urgent and could wait. Then, toward the end of the Bents' expedition, the overall situation is no less clear and evident. "Stricken with fever, and worn out, the party reached Aden, and Mr. Bent died a few days after his return to England," states The Outlook in Press Notices, p. xii, ll. 17-19; Mrs. Bent had already said in BeSA, p.v, ll. 3-4, that "he died four days after our return from our last journey there."

It is, therefore, almost manifest that Mr. Bent himself could not have sent the squeeze from either Aden or England. Who, then, but Mrs. Bent sent the squeeze to Glaser after her husband's death? The time and the manner of the dispatch of the squeeze are irrelevant to the sending itself.

b - The SECOND section (p. 34, ll. 17-22) reads as follows: "Secondly, the reproduction of the critical photograph from the Bent book [9] printed in BULLETIN 196, p. 41, is inadequate to show that the enlargement on p. 43 and again in BULLETIN 199, p. 62, was incorrectly trimmed so that the protrusion on the break at the tip of the *m* was removed, minimizing the similarity to the photograph in BULLETIN 109. p. 63 (captioned "The Bethel stamp")."

The only acceptable information on the Bent stamp is its photo published in BeSA, and all study of the stamp must be based upon it, viz. it is unscientific to base a vital argument on any reproduction of the original. The description of BASOR, No. 199, p. 63, is based on the study of the photos of the Bethel stamp in BASOR, No. 151, October, 1958, p. 11, fig. 2 = No. 163, October, 1961, p. 15, right, and that of Bent stamp published in 1900; the text of BASOR, No. 199, p. 63, reads as follows: "Notice that the line of fracture is different on the two stamps. The oblique fracture line of the Bethel stamp is almost straight, whereas that of the Bent stamp is slightly convex. The Bethel stamp also broke differently in the upper right corner, above the letter "m," where the line of fracture barely rises above and almost parallels the horizontal line divider between lines 1 and 2, while on the Bent stamp the fracture continues upward to an apex and then drops off toward the right edge. Note especially that the Bethel stamp is preserved *higher* in the upper right corner than the Bent stamp."

Cleveland's putting the question on a siding is an easy way out and a lame excuse for avoiding the objective study, but it does not make the latter go away.

c - The THIRD point (ll. 23-32) raised by Cleveland is as follows: "Thirdly, by not commenting on the conjecture in T. C. Mitchell's letter that the stamp may have been deliberately rejected by the British Museum in 1926 when other objects in the photograph were presented by Mrs. Bent," Yadin rather implies his belief that it was in England until that time. Jamme and Van Beek transformed Yadin's implication into an explicit assertion: ". . . the stamp . . . must have come to the site [Beitin] after A.D. 1926. . . ." [10] No evidence indicating that the group of objects photographed in the late 1800s was still intact in 1926 has been cited. The three objects in the photograph not given to the Museum may no longer have been in Mrs. Bent's possession by 1926.

I am still confident that our interpretation of Yadin's opinion is correct. However, such a question is strictly academic since Yadin did not clearly express his view on the matter; an unspoken idea of Yadin is of no importance in the case. Such already was the opinion of BASOR, No. 199, pp. 59-65, where Yadin's opinion is not taken up at all. Therefore, there is no justification in mentioning such a valueless point as if it were a determining element of the problem ... unless it is an attempt at confusing the whole question by putting it on a second siding.

d - The FOURTH point (p. 34, l. 33 - p. 35, l. 4) brought up by Cleveland reads as follows: "Fourthly, the rejoinder by Jamme and Van Beek argues that the deliberate placing of the clay stamp at Bethel would have required someone knowledgeable in the archaeology and history of both South Arabia and Palestine, as only such a person would have been aware of "the historical and cultic significance inherent in the seal." [11] That argument overlooked the possibility that Beitin could have been chosen for burial of the object not because of the object's historic significance, but because of the significance someone possessing the stamp attributed to it and to Bethel. The motive was not necessarily knowledge in the scholarly sense."

Cleveland's reasoning is a good example of a vicious circle. Indeed, he gives as a possible explanation, which is one of the "comments ... [which] have tidied the setting sufficiently" (p. 35, ll. 17-18), the summary itself of the gratuitous scheme which he is going to formulate later. Furthermore, such an explanation is unacceptable because, as it is presented, it appears to be a quite normal and logical solution. But, it is neither logical nor normal to postulate a solution involving an accusation of fraud or dupe leveled against a respectable lady. It certainly never crossed my mind that such aberrant a solution could be devised since the archaeological facts and/or Mrs. Bent's writings do not contain the slightest hint justifying even the hypothesis of what is, in my opinion, a shameful scheme.

e - The FIFTH and last point (p. 35, ll. 5-16) raised by Cleveland reads as follows:

[A] "Finally, the comparison of photographs by the discussants is not entirely persuasive. For instance, the angle from which the cast of the "Bethel stamp" was photographed [1] does not show the curvature of the break which Van Beek clearly indicated in his drawing which accompanied his original publication.[2] The alleged absence of this feature was later used as an argument against the single-stamp theory.[3] The discrepancy between drawing and photograph suggests that the photograph of the "Bethel stamp," intended to be "posed at the same angle and lighted in approximately the same manner as the Bent stamp," in fact is not leaning back as much as the latter. More significantly, it casts some question on the possibility of distinguishing two objects on the basis of the evidence in hand." [B] [C]

The three notes involved are as follows: " [1] Ibid., p. 83, Fig. 2.
[2] Bulletin 181, p. 14, Fig. 4, right.
[3] Bulletin 199, p. 83."

Ad [A] - On the one hand, the reader wonders why Cleveland chose, for his study, the photo of the cast of the Bethel stamp (cf. BASOR, No. 199, fig. 2) instead of that of the original itself (cf. BASOR, No. 151, p. 11, fig. 1, and 163, p. 15, right). Here again, Cleveland did not use the best documentation for his study. On the other hand, the reader also wonders why Cleveland did not inform his reader that the "drawing" of BASOR, No. 151, p. 14, right, actually is, and is presented as, a "Reconstruction of the stamp." A "reconstruction" is a model or replica of an original which never is its minutely accurate reproduction. The shape of the break is traced from the original as well as possible. Finally, I personally have no difficulty when looking at the photo of the original and at the cast itself to recognize the bonds of the facsimile.

Ad [B] - Contrary to Cleveland's statement, the break of the stamp would have been seen in a better light had the top of the cast been tilted more frontward than it was when the photo was taken.

Ad [C] - Cleveland's statement is not a deduction from his so-called first-hand study, but in fact is the presentation of his preconceived position which was demanded as the necessary prerequisite by the scheme he is about to develop, viz. it is another vicious circle.

C - Cleveland and Mrs. Bent's booklet.

1 - Bibliographical introductory note (p. 55).

The title of Mrs. Bent's booklet is given by Cleveland as follows (p. 55, ll. 20-21 and 25): "*Anglo-Saxons from Palestine: Or the Imperial Mystery of the Lost Tribes*....London in 1908."

Cleveland limits himself to speaking of
"a little book" (l.19),"78 pages of text in this little book" (ll. 43-44), "The fourteen pages of press notices of Southern Arabia reprinted as an appendix in the later book" (p. 56, ll. 8-10).

The title of Mrs. Bent's booklet is"Anglo-Saxons from Palestine
Or the Imperial Mystery of the Lost Tribes (p. 111).

The booklet had two editions during the same year 1908; and the first edition had 70 pages only (cf. BMCC, vol. 14, 1965, col. 1432).

The first deduction is that Cleveland erroneously inserts ":" after "Palestine" in his transcription of the text. It should, however, be noted that BMCC, l.c., lists the book as "Anglo-Saxons from Palestine: or, the Imperial mystery of the lost tribes" although there is no punctuation in the original. The National Union Catalog Pre-1965 Imprints (vol. 47, 1969, p. 306B) copied BMCC, but replaced "the Imperial" by "The Imperial." The liberty Cleveland takes in quoting a title is also noted in his transcription of another title,"Forty-seven Identifications" (p. 55, l. 26; not in italics as the other titles), but the uppercase "I" becomes a lowercase,"i," three lines further down, ll. 29-30.

The second deduction is that Cleveland used the second edition only.

The third deduction is that, by presenting the "Press Notices" "as an appendix," he actually incorporates those pages to the book, an "appendix" being a "matter added to a book but not essential to its completeness (as a bibliography or a series of tables usually following the text)." In fact, those notices do not in any way belong to the booklet; they are excerpts of press reviews of BeSA prepared by the publishers for publicity purpose, viz. they are aimed at extolling Mrs.Bent's achievements as both an explorer and a writer. It would have been an impossible task, obviously enough, to expunge all mention of Mr. Bent, but the printers' aim is evident since Mr. and Mrs. Bent are mentioned or referred to 87 and 95 times, respectively, in that selection of press reviews.

The fourth deduction is that Cleveland fails to mention another addition to Mrs. Bent's booklet by the same publishers, viz. 24 pages of the..." CATALOGUE
This catalog contains 40 titles. Since those two sections
are not parts of the booklet, BMCC does obviously not list
them in the title. Cleveland's utilisation of the first in
connection with Mrs. Bent's writing is a distortion of the
booklet, and his elimination of the second is illogical sin-

ce he mentions the first.

2 - Preliminary note.

The "new clues" or "further clues are mainly found in " Mrs. Bent's booklet. In the remaining 1 1/2 pages of his paper (pp. 35, l. 20 - p. 36, 44 lines), Cleveland does not utilize or refer to any other source of information which might have provided him with any of his "clues." Therefore, since his adverb "mainly" remains unjustified, the reader may safely conclude that the use of that adverb is a part of the overall window dressing aimed at creating an impression of strength and diversified evidence for his theory at the expense of objectivity.

3 - Cleveland's long argumentation on the date of Mrs. Bent's acceptance of the belief in the so-called British Israelitism is an excursus having no direct bearing on the question at hand. However, it brings about other major difficulties against his theory, which are not even mentioned.

a - On pp. 73-75, Mrs. Bent lists 13 pamphlets and

> "a Magazine called "Covenant People," which is the official organ of the Imperial British Israel Association, which holds its meetings every month at Caxton Hall,"

and Cleveland accurately states that Mrs.Bent

> "credits her conversion to British Israelite beliefs to "books" by Edmund Hine. mainly" (p. 35, ll. 25-26) to a pamphlet which "was first published in 1874" (p. 35, l. 30).

b - Then, Cleveland adds this:

> "another title she cites is [A] more useful as a *terminus a quo* for her conversion, the writing of her book on the subject, and for her sojourn in Jerusalem. "Coming events [B] in the East. or, Return of Jews to Palestine," by H. Aldersmith (earlier Herbert Alder Smith) would most likely have been available to her in an 1896 or later edition" (p. 35, ll. 30-35).

Cleveland continues feeding his reader with mere statements without even the hint of a proof.

Ad [A] - Why would H. Aldersmith's pamphlet be "more useful" to Mrs. Bent's "conversion," etc? I am lost in conjecture as to what that reason might be. However, when that suggestion is replaced in the context of the whole argumentation, it becomes obvious that Cleveland had to insert it in order to surreptitiously inject a late "terminus a quo," such as "an 1896 or later edition," into his reader's mind.

Ad [B] - The title of H. Aldersmith's pamphlet is listed as follows in BMGC, vol. 224 (1964), col. 390:

> "SMITH (Herbert Alder) afterwards ALDERSMITH (Herbert)
>
> —— Coming Events in the East and the return of the Jews to Palestine . . . Reprinted from "The Covenant People" (Third edition.) pp. 20. J. Nisbet & Co.: London, 1896. 8°. 20019 c. 37"

The title of the pamphlet, such as given by Cleveland, is copied from Anglo-Saxons (p. 73) and is not exactly as in BMGC. Furthermore, the 1896 date given by Cleveland was copied from BMGC; but, contrary to Cleveland's statement, no "later edition" is mentioned. The booklet was a reprint of "The Covenant People," 3 (1896), since this bulletin started in 1894 (cf. British Union-Catalogue of Periodicals, vol. 3 [1957], p. 189A).

Cleveland's choice of the 1896 date gives another proof of the preconception of his overall thesis because Mrs. Bent could still have read the pamphlet before departing for her last trip to South-Arabia. Therefore, what is the advantage of retaining such a date since his thesis calls for her conversion to Anglo-Israelitism after that last trip. But, his choice of that late date was prompted by the fact that he had no other choice, viz. in all the pamphlets listed by Mrs. Bent on pp. 28, 63 and 74, there is only one with a later date which, in fact, is too late, viz. 1908 for W.H.H. Milner's pamphlet entitled The Royal House of Britain an Enduring Dynasty ... (cf. BMGC, vol. 160, 1963, col. 871). Such a date was obviously excluded because Mrs. Bent's booklet was published the same year 1908. Summarily, one date only is certain; that which indicates the "terminus ante quem non" of her conversion to Anglo-Israelitism, viz. 1874 (cf. above, p. 144).

c - Then, Cleveland states that

> "Her conversion, furthermore, must have taken place after 1899, as there is no hint of British Israelitism in *Southern Arabia*."

The use of the argument "ex absentia" is valid only when it is proven that the absent information should have been given in the first place; and this Cleveland does not, and cannot, prove. Mrs. Bent herself declares in the "Preface" of her booklet (p. V, ll. 6-9) that " Of the past and of the present I have come. I have tried to mind my own business, spoken and said little of the things for to which is that of the Anglo-Saxons."

Therefore, BeSA was not her "own business" but that of both her husband and herself. And since her affirmation is unequivocal, viz. "I have tried to mind my own business," it is clear that she did not refer, and had no intention of referring, to her personal belief in BeSA because the trips described in that book were not her "own business." The preceding remark applies also to Mrs. Bent's paper published in 1898 in The Geographical Journal (cf. below), a paper which Cleveland

carefully avoids mentioning at all although, being written by Mrs. Bent immediately after her and her husband's return from Arabia and even before the composition of her booklet, that very paper is of extreme importance to judge her psychological condition which is, after all, the sole basis of Cleveland's theory. It is, therefore, obvious that Cleveland's statement is not only unfounded but also contradicted by Mrs. Bent's personal testimony. Furthermore, Mr. Bent's personal attitude toward his wife's belief is of no interest here; it suffices to point out not only that the purpose of the Bents' adventures had nothing to do with that belief and also that both Bents were logical enough to keep apart and unrelated their expeditions to the Near East and Mrs. Bent's personal belief.

d – The preceding facts and argumentation prove how preconceived and unfounded is Cleveland's last statement in the matter, which immediately follows his aforesaid (under c) quotation and which reads as follows (p. 35, ll. 37-38): "In any case, she can safely be placed in Jerusalem after the last journey to the southern shores of the Arabian Peninsula"

Note first that Cleveland's "she" means Mrs. Bent's conversion! Furthermore, the preceding statement is a story of Cleveland's own invention, and the reader can, therefore, judge by himself the meaning given to the adverb "safely."

4 – Features of Mrs. Bent's booklet according to Cleveland.

a – The initial feature of Mrs. Bent's booklet as given by Cleveland is this (p. 35, ll. 39-41):

"Jerusalem, Hebron, Mizpah, and Bethel are the only biblical towns mentioned in *Anglo-Saxons from Palestine*. Bethel is mentioned by name only once."

Unless an unusual meaning is given to "biblical towns," it is impossible to understand why, beside Zion (p. 64, l. 16, twice), the six following names are excluded from the preceding list, viz.

Tekoa "p. 33, l. 13): a Judaean town and Amos' native country; about 8 klm south of Bethlehem;

Beth-haccerem (p. 33, ll. 14-15): probably Ramat Rachel; just south of Jerusalem;

Pella (p. 34, l. 4): opposite the Beth-shan gap on the Transjordanian side of the valley, assigned to a portion of Manasseh;

Jezreel (p. 44, l. 7): town of Issachar, standing at the western entrance to the corridor leading to Beth-shan;

Megiddo (p. 50, l. 21): town attributed to Manasseh, a district capital of Solomon; and

Shiloh (p. 51, l. 5): town in the heart of Ephraim's territory.

All those names are mentioned once each with the exception of Jerusalem, Zion, the names of which appear fourteen times (pp. v, 4, 8, 9, 13, 27, 33, 34, 45 [twice], 50 [twice] and 64 [twice]). In good logic, Jerusalem must have been for her by far the most important town where she sojourned while being in Palestine. It must also be pointed out that Mrs. Bent never said that she traveled to Bethel or to any other town than Jerusalem.

b – Cleveland's next statement is as follows (p. 35, ll. 43-45):

"The significant thing is that the burden of the 73 pages of text in this little book is that the British are the descendants of the "Ten Tribes of the House of Israel." "

Cleveland's summary of the booklet is inaccurate. Mrs. Bent herself declares that her booklet "is a mere A B C, but I have purposely kept within narrow limits," (p. v, ll. 1-3) of a belief which "is no new one" (p. 3, ll. 3-4) and "is a growing one" (p. 3, ll. 7-8). Therefore, Mrs. Bent's intention was not to break in new ground to prove the existence of a belief because it was not new; what she wanted to do is to simply present a succinct summary (cf. her own words, viz. " I have purposely kept within narrow limits"), a "mere A B C" of the well-known biblical basis of that belief.

c – Cleveland, then, states that (pp. 35, l. 45 – 36, ll. 1-4)

"She belabors the point that [A] the House of Israel has always been distinct from the House of Judah." [B] Thus Bethel would presumably have had unusually great interest for her; while Jerusalem was the capital of Judah, Bethel was the center for the [C] House of Israel."

Footnote 20 (p. 36) refers to "pp. 4 and 7-12" of the booklet.

Ad [A] – On p. 4, ll. 5-7, Mrs. Bent states simply that "the House of Israel is not amid the House of Judah at the present time."

On p. 7, l. 21 – 8, l. 4, Mrs. Bent affirms that

"Though the Houses of Israel and of Judah were finally separated under the Kings Jeroboam and Rehoboam, the distinction between them began before that."

The expression "before that" does obviously not, and cannot, imply the meaning of "before that to the beginning of history inclusively." Besides, Mrs. Bent reproduces 30 quotations from the Old Testament (pp. 8, l. 4 – 12, l. 7), but none of them precedes the period of the Judges. Mrs. Bent's deduction from her 30 quota-

tions, viz." It being thus manifest Israel and Judah were distinct " (p. 12, ll. 15-16) does not, and cannot, imply more than what they express because her statement does not contain the adverb "always." It is, thus, obvious that, here again, Cleveland distorts Mrs. Bent's testimony by injecting a generalization which is inexistent in her text.

Ad [B] - This is another mere statement attributing to Mrs. Bent an "unusually great interest" on Bethel. Although demanded only by his preconceived theory, such a claim is not supported by any of Mrs. Bent's writings; on the contrary it is contradicted by the number of references to Jerusalem and Zion (cf. above, p. 147).

Ad [C] - The parallelism between Jerusalem and Bethel is ridiculously simplistic; cf. e.g., R. North in The Jerome Biblical Commentary (Englewood Cliffs, N.J., 1968, p. 650A, par 73:98)

"Bethel served as a shrine and a rallying place in the time of the judges (Jgs 20:18); and in an attempt to return to "that old-time religion," Jeroboam I, after the schism of 922, made Bethel, along with Dan, one of the national shrines of the northern kingdom, counteracting Davidic and Judahite pretensions at the upstart shrine of Jerusalem."

5 - From here on, Cleveland turns his attention to Mrs. Bent's psychological frame of mind in relation with her booklet and the clay stamp.

a - His f i r s t argumentation is as follows (p. 36, ll. 6-8):

"The confused arrangement of ideas is completely different from the systematic organization of *Southern Arabia*."

Is it not strange to see now great emphasis put on "the systematic organization" of BeSA when it is stated above (p. 137) that

"One may not safely attribute the same expertness to all parts of the book or expect uniformity."

Here again, Cleveland distorts the facts: Mrs. Bent's so-called "confused arrangement of ideas" exists in Cleveland's mind only as the deduction from his next statement (cf. below, "second affirmation").

(1) Mrs. Bent's argumenting method is both logical and easy to follow.

(a) When she indulges in a "digression," she knows it and spells it out; cf., e.g., (p. 26, ll. 1-3)

" Let us now return to King David's family after this digression about the Coronation stone."

(b) She is honest and objective, viz. she is not blinded by her belief; e.g., she writes (p. 34, ll. 10-14) that

"Here are produced a few of the very many strong reasons, negative as well as positive, for feeling sure that the Ten Tribes and the British people must be one and the same."

Therefore, Mrs. Bent shows herself quite capable of seeing the difference between what she calls the "reasons, negative as well as positive."

(c) Mrs. Bent is not afraid of dealing openly with objections against her belief; e.g.

"One of the objections set forth is that the English are a mixed race " (p. 51, ll. 20-21) and

"The Jews seem to consider that the Promised Land consists solely of Palestine " (p. 53, ll. 4-6).

(2) The basis of Cleveland's judgment is anachronic: he judges Mrs. Bent's argumenting method on the basis of the present-day rational reasoning which was unknown to her since she received her formal education in the 1860s-early 1870s as she got married in 1877 (cf. Who Was Who 1929-1940, London, 1941, p. 98A). Furthermore, Cleveland's attitude is biased: even today many persons, professional intellectuals or not, still use crooked and distorting arguments to more surely attain their goals or prove their point or sell their theory, and an outstanding example of such a behavior is Cleveland's paper discussed here. Moreover, if Cleveland's statement were correct, he should have explained how Mrs. Bent's booklet had two editions in the same year 1906; how

"A great many books have been written on this subject " (p. 72, ll. 8-9); how the 14 booklets listed by her (pp. 20, ll. 16-17 and 73, l. 5 - 74, l. 16) were so popular since, as Cleveland himself admits,

"These "books" were apparently only tracts which enjoyed numerous reprintings and revisions" (p. 35, ll. 28-29); and how "the Imperial British Israel Association" (p. 75, ll. 1-2), of which "the offical organ" (pp. 74, l. 21 - 75, l. 1) was "a Magazine called "Covenant People."" (p. 74, ll. 20-21) (cf. already above, p. 144) could exist at all and even lasted until after WWI since it was in 1921 that it merged in The National Message, 1 (1922) (cf. British Union-Catalogue of Periodicals, 3 [1957], p. 189A). Summarily, if Cleveland's statement is accurate, there must have been tens of thousands of British feeding on mere "tracts" composed of "confused arrangement of ideas" ever since, e.g., 1377 (p. 1, l. 10), and many of them had received a very fine education setting them distinctly above the intellectual level of either an ordinary peasant or a street sweeper or a simple docker.

b - Cleveland s e c o n d affirmation appears to be the basic foundation of his thesis, viz.

"She was a widow going through a great emotional crisis" (p. 36, l. 8).

Here again, Cleveland speaks out of his head, viz. how does he know what happened

after Mr. Bent's death on May 5, 1897?; how does he know that there actually was "a great emotional crisis"? A very deep sorrow is not necessarily "a great emotional crisis." And, even taking for granted that there was one – but Cleveland only has his preconceived thesis and nothing else to support his claim –, how can he prove that Mrs. Bent still remained in that psychological condition for more than t e n full years? It is indeed evident that, for Cleveland, Mrs. Bent was still in that mental condition in 1907-1908, when she completed the writing of her booklet. But, how could Mrs. Bent's mental sanity be maintained all through ten long years of "great emotional crisis"? The truth of the matter is that no sign whatsoever of that alleged "great emotional crisis" can be found in either BeSA or even her paper entitled "Excursion in the Yafei and Fadhli Countries" printed in The Geographical Journal, 12 (1898), pp. 41-63 (cf. already above, pp. 145-146). The three most important features of this paper are these.

a – Mrs. Bent's writing of the paper took place immediately after her husband's death (cf. also The Outlook in Press Notices, p. xii, ll. 15-17 "The last expedition, that to the Fadhli land, cost him his life;" cf. already above, p. 140). The testimony of the last two phrases of that paper is clear and irrefutable; they read as follows (p. 63, ll. 28-20):

> "This is all I can write about this journey. It would have been better told, but that I only am left to tell it."

How is it possible to reasonably infer "a great emotional crisis" from the last two remarks of her paper? They factually present concisely her actual situation, viz. being left alone, she is not in the ideal position of recording comments, views and details which her husband might have had during that trip. Furthermore, Mrs. Bent's writing had to take place immediately after her husband's death; otherwise, the paper could not have been printed as the second ordinary paper in the first issue, July of vol. 12 (1898) of the journal, while two communications "read at the Royal Geographical Society", February, 28, 1898" and "read ...,March, 14, 1898," respectively, fill the first pages of the second issue, August, of the same vol. 12 (cf. pp. 113-137 and 137-138, respectively). Obviously enough, Mrs. Bent could hardly have wasted any time after her husband's death, indulging in self-pity, brooding and/or "going through a great emotional crisis," as Cleveland puts it: she wrote a paper for The Geographical Journal; she did the preparatory work for her next booklet; she started the writing of her booklet; she went to Palestine; she completed the writing of her forthcoming booklet; she went back to England; she saw her booklet through the printing process in 1908 and a second edition the same year ... and that long series of demanding activities could hardly have begun before May 20, 1897, since the first weeks after May 5, 1897 (her husband's death), social activities, such as visits to and from relatives and close friends and the unescapable legal work to finalize her husband's testament, must have taken a heavy toll of her time.

b – There is not the slightest allusion or reference, whether implicite or explicite, to the belief in Anglo-Israelitism in her paper because, as stated above (p. 145), the trip described was not her "own business."

c – The style is clear, matter-of-fact, and easy to understand.

Therefore, the writing of those two publications covers a period of months starting immediately after Mr. Bent's death; and both publications witness to a rational and factual thinking quite alien to Cleveland's concoction of an alledged "great emotional crisis."

c – Cleveland's last statement relating to BeSA reads as follows (p. 36, ll. 8-12):

> "The fourteen pages of press notices of *Southern Arabia* reprinted as an appendix in the later book also indicates that she was living in the shadow of the achievements of her late husband as she had brought them to public view."

As already demonstrated above (pp. 143-144), Cleveland's utilization of the "Press Notices" is a gross distortion of the facts, and his remark amounts to flailing the air. Furthermore, Mrs. Bent lived so much "in the shadow of the achievements of her late husband" (cf. similar expressions above, p. 139), as Cleveland claims, that NOT ONLY she n e v e r refers to her late husband or to her previous marriage in her booklet BUT ALSO she declares herself in the front page of that booklet to be the s o l e "Author of Southern Arabia, Soudan and Sokoura, &c." (p. III; cf. already above, p. 136).

d – Cleveland summarizes his psychological portray of Mrs. Bent in these words (p. 36, ll. 13-14; cf. already above, p. 139):

> "Given Mrs. Bent's anxiety during those years and her clinging to the memories of her husband."

The preceding paragraphs show not only that Cleveland never gave a single fact in support of his claim but also that there is no "anxiety" at all detectable in her writings. Here again, the only basis of Cleveland's statement is his own preconception built up o u t s i d e Mrs. Bent's publications.

e – Cleveland's following statement is the hinge connecting his psychological picture of Mrs. Bent with the case of the stamp. He wri-

tee this (p. 36, ll. 14-15): "it would have been most remarkable if she did not carry with her little mementos of her former conjugal happiness." Here again, Cleveland speaks out of his head. Nothing is known of Mrs. Bent's personal habits, and this is all the more certain that even Cleveland admits that (p. 35, ll. 23-25) "Information about her activities after the completion of *Southern Arabia* in 1899 is fragmentary."

In fact, the only detail of her social life after her return from South-Arabia is given by Mrs. Bent, viz. her booklet "written chiefly in Jerusalem;" Cleveland refers to it on p. 35, ll. 21-22. Therefore, if Mrs. Bent was already so secretive about her social life, how can anyone expect her to give details on her private life and in particular on her feelings and on her way of expressing them? What kind of "fragmentary" information does Cleveland have on Mrs. Bent's private life? He does not give any detail! Had he given any, the reader would have known the nature, social or personal, of that "information." It is presumptuous as well as tactless to pry into and/or speculate upon the psychological affairs of any human being. Moreover, here again, Cleveland distorts the reality by describing a single detail of social life as in information on private life, and also by presenting it as a "fragmentary" "information," an expression which cannot but lead the reader astray by giving him the impression that there are at least a few, scattered details available, which is not true. Finally, women do not react uniformly in widowhood. If some widows wear their husband's wedding ring or a medaillion or a pin given to them by their late husbands, others do not wear anything special, and who has ever read, or heard of, a single case of a widow who did "carry with her" a reminder of her late husband? The reaction of any given widow is unpredictable as it is a part of her strictly personal behavior. Summarily, Cleveland presents his own preconceived idea on a very personal question without the slightest proof or even hint from Mrs. Bent herself for the sole purpose of introducing his prefabricated understanding of the case of the clay stamp.

f - Cleveland is now ready to introduce the first prerequisite of his detective saga of the Bethel stamp. He writes (p. 36, ll. 16-19): "In her travels, she would not likely have taken heavy stone objects such as those of the photograph group which eventually reached the British Museum in 1926, but smaller and lighter ones, such as the broken clay stamp and the two smaller seals which are now unaccounted for."

The preceding statements are empty babblings because Cleveland never proved that the Bents ever possessed or owned those three objects. The analysis of the Bents' narrative indicate that they never did own those objects (cf. already above, pp. 138-139). Furthermore, it is ridiculous even at first sight to think that Mrs. Bent would have taken along with her in her luggage the clay stamp which at any rate was too heavy and too bulgy to be either hung from her neck or carried in her handbag. Moreover, Cleveland does not even attempt to justify why Mrs. Bent would have taken along with her the three seals; one of them would have been quite enough. Finally, far from being gifts of her late husbands, they could hardly have had any sentimental value for her; on the contrary, they were reminders of their last trip which had brutally put an end to her married life. How is it possible to view such objects as "her little mementos of her former conjugal happiness," as Cleveland puts it (cf. already above, p. 152)?

6 - The second and last prerequisite of Cleveland's imaginative saga is formulated as follows (p. 36, ll. 20-21):

"The question then is whether she might have left these three objects in Palestine and Egypt? (She was also strongly attracted to pyramidology.[1])"

This question appears to be a mere window dressing because Cleveland excludes other possible places without even mentioning them and/or justifying his exclusion of them. He does not even mention the possibility of the Bents having, voluntarily or not, left the three objects behind in South-Arabia or of Mrs. Bent having disposed of them in England. He introduces the name of Egypt although there is no information available that Mrs. Bent traveled to Egypt after her departure from South-Arabia. In fact, it is really astounding that Cleveland should explicitly refer to Mrs. Bent being "strongly attracted to pyramidology," with his footnote 21 referring to "pp. 88-93" of the booklet. He does not seem to realize that, by doing so, he himself gives the reader another indisputable proof of the preconception of his whole theory. Cleveland himself declares - how could the reader forget it? (cf. already above, p. 146) - that "Bethel is mentioned by name only once" (p. 35, ll. 40-41). How can someone understand the logic of Cleveland who would center his whole theory on a single name mentioned once only (p. 24, l. 11) in a context which Mrs. Bent herself calls a "digression" (p. 26, ll. 2-3; cf. already above, p. 148) of 38 lines, without even giving a single word of explanation for his quiet dismissal of his hypothesis of "the Great Pyramid" (p. 59, l. 1) to which 111 lines of the text (pp. 58, l. 19 - 63, l. 11) are devoted? Indeed, the reader in vain seeks any comment on the question itself and/or any reason why the Egypt hypothe-

sis is silently dropped. Finally, this is the last time that Cleveland speaks of the "three objects." Therefore, what is the reason for mentioning them here again? By reminding his reader of the existence of the two other seals, Cleveland himself voluntarily gives more fuel to one of the reader's criticisms. Those two seals, too, are parts of the whole problem and, therefore, Cleveland should have explained on the basis of his analysis of Mrs. Bent's psychological condition why she actually chose the stamp rather than either seal. In fact, the choice of the first rather than of either of the two others would be incomprehensible for the following reasons. a - The seals were intact, but the stamp is fragmentary as almost the upper half of it is still missing;

b - nobody in his right mind would commemorate a loved one by using a broken-off object preferably to a complete one;

c - the fragmentary condition of the object would only necessarily symbolize and/or remind her of the abrupt end of her "conjugal happiness;"

d - one small seal of unspecified material (probably a semiprecious stone) still had "its golden setting," and the other was in "copper" (cf. already above, p. 138 for BeSA's text). The two seals were pretty to look at; the stamp was ugly. Who in his right mind would prefer a broken earthen stamp to either seal and especially the one "with its golden setting"?; and

e - according to the same text of BeSA, the two small seals had an inscription which Mr. Bent apparently could read since the book gives their translation, but the clay stamp was covered with letters which are not even mentioned in BeSA as if Mr. Bent did not recognize them as letters.

Therefore, had Mrs. Bent any choice, she most certainly would have retained either seal and preferably the one "with its golden setting." But, she never had such a choice because her husband and herself never acquired or owned any of those three objects (cf. above, pp. 137-138).

The argumentation of the whole paper makes it certain that the two small seals have no place in Cleveland's preconceived scheme, and their sudden elimination, as if they were inexistent, is another proof of the preconception of his whole saga.

7 - Cleveland's evaluation of his own thesis is expressed as follows (p. 36, ll. 22-23): "One may venture the following reconstruction, which though somewhat circumstantial and speculative is nevertheless possible." According to the author's own avowal, his theory is not at all a logical deduction inferred from an objective analysis. Even from the outset, his theory is nothing

but some kind of fictitious saga or speculative concoction or imaginative novel and to declare such a fantastic scheme "possible" is to defy good common sense and the basic principle of scientific research, viz. the respect of all relevant information. The preceding pages have indeed demonstrated that the contents of Mrs. Bent's booklet contradict Cleveland's preceding statement.

8 - Cleveland's thesis is presented as follows (p. 36, ll. 24-35):

[A] "Convinced that ancient Israel was the origin of "the British race" and devoted to the memory of her late husband, Mrs. Bent buries a symbol of herself and Theodore, some object deriving from their joint endeavors, in the cultic center of the House of Israel, the home of the covenant [B] [C] [D]
[E] "Britain means the land of the Covenant, from *Brith* . . .")." She does [F]
not simply leave the token exposed on the surface, knowing that it would be carried off and sold as an antiquity. Using some pretext, she has a hole dug, then conceals the object in the bottom and has the cavity
[G] refilled. That is a hypothetical model. If something like this actually [H]
happened, the fill from which the clay stamp was recovered in 1957 would then have been the disturbed debris of Mrs. Bent's pit, containing only one extraneous artifact, the stamp itself."

Even the simple reading of the preceding saga, which the author himself characterizes as "a hypothetical model" (cf. [G]) or subjective dummy, actually charges a respectable lady of a voluntary fraud, unheard of in archaeological history, or imposture without the slightest evidence and on the sole basis of his own preconceived theory. It is, indeed, appalling that such a scheme could be thought of and made public in the pages of a respectable scientific bulletin almost entirely devoted to archaeological studies.

Ad [A] - This is the first accurate statement of the whole argumentation, and it is proven by [E], the second and last accurate affirmation, on the meaning of Britain, which may be summarized as follows: Hebrew b°rît becomes brit + ish/ain. The flimsiness of such an argument is obvious, to be sure. But, while characterizing Mrs. Bent's argumentation of her booklet as a "confused arrangement of ideas "(cf. above, pp. 148-149), Cleveland offers a paper of much worse quality since his preconceived scheme is either contradicted by, or dreamed beside, the very booklet upon which it is claimed to be founded. And Cleveland's paper is but one example of that type of erratic thinking. Among the many other examples denounced in this volume, the three following specimens deserve a very special mention, viz. A.F.L. Beeston's, M.A. Chul's and J. Ryckmans' nonsensical etymologies

of [the] of Ja 3200/1 (cf. above, pp. 94-95, 94 and 99-101, respectively).

Ad [B] - Mrs. Bent's actual psychological state is not, and will never be inasmuch as I am concerned, the object of any inquiry. What is at stake here is what transpires from her booklet. As demonstrated above, what transpires from her booklet is twofold.

a - There is a complete blank on her state of mind toward her husband; such a situation proves only that she did not wish her feelings to be made public and did not give any hint as to their nature and intensity. Any speculation about them shows bad taste and violates a basic right of the individual (cf. already above, p. 152).

b - The o n l y fact manifested by Mrs. Bent is h e r exclusion of her late husband's name as coauthor of BeSA (cf. already above, pp. 136-137), and her decision was her concern, not ours. This literary evidence openly contradicts Cleveland's statement and, therefore, positively eliminates the personal motivation of the alleged fraud of which he falsely accuses her.

Ad [C] - Cleveland does neither prove nor explain how a b r o k e n clay stamp could be "a symbol" of a married couple, "some object deriving from their joint endeavors."

1 - This antiquity is not even intact since almost the upper half of it was already, and still is, missing (cf. already above, p. 154). It was neither excavated nor even found by the couple during a spectacular act of bravery. Shown by an unknown native during an uneventful, undated day, such a fragmentary piece of pottery is the worst "symbol" of a married couple that could have been chosen by Cleveland since it was seen by the couple during their last trip which costed Mr. Bent his life (cf. already above, p. 154).

2 - It is simply irrational to think that a pre-Islamic stamp, whether from the north or from the south, could be connected with Anglo-Israelitism even if it were excavated in Palestine. Such an object should at all cost be specifically British; otherwise, the connection is unthinkable.

3 - Quite unexpectedly, Cleveland indirectly connects the belief of Anglo-Israelitism with Mr. Bent. This new idea is most surprising since it is contradicted by his previous position, viz. he himself stresses the point that there is no trace of Anglo-Israelitism in BeSA and also that Mrs. Bent's conversion to that belief did not occur before 1899 (cf. his text above, p. 145). This is another example of Cleveland's illogism because, as it is well-known, Mr. Bent died on May 5, 1897. However, if, by any chance, Cleveland does not, by another illogism, connect Mr. Bent with Anglo-Israelitism, what reason does he have to imagine Mrs. Bent posing an act by which she would connect her late husband with a belief she knew he never had?

4 - Only a demented person would think of a stupid fraud, such as that of which Cleveland accuses Mrs. Bent because that very person not only published a few years earlier the photo of that object but also would a few years later remind the scientific community of the existence of that object by presenting to the British Museum most of the antiquities shown on that very photo.

On the one hand, since Mrs. Bent appears in her booklet Anglo-Saxons to be of sound judgment and not at all demented or unsound or irresponsible, how can Cleveland dare to accuse her of a fraud unique in the annals of archaeology? It is quite impossible to understand that Cleveland would think Mrs. Bent to be so stupid as not to realize that her fraud could - and probably would - be discovered sooner or later - it was only a matter of time, even if it were 200 years after her fraudulent act - because of the photo that her late husband and herself published in 1900 and, therefore, that she would f o r e v e r carry the brand of committing a fraud. It is also quite impossible to understand that Cleveland, after repeatedly asserting how devoted the widow Mabel Virginia Bent was to her late husband; how much she lived immersed in his achievements; how much she clinged to his memory, could not have seen even at first sight how preposterous it is to even think that such a wonderful widow would deliberately and forever stain her late husband's memory by a fraudulent act because Mr. Bent would henceforth be known as the late husband of a ... cheater.

On the other hand, if by impossible she was certain that the stamp would never be discovered, she knew by the same token that her act would never yield the very effects she intended them to return, viz. to publicly show both her faith in Anglo-Israelitism and her devotion to her husband. But, such goals would have been quite unnecessary and superfluous. The two editions of her booklet already made the first point unmistakably clear, and the writing of her paper of The Geographical Journal and BeSA proved her resolution to publish the results of their joint expeditions. However, the restriction of her ascribing to herself alone the authorship of BeSA (cf. above, p. 136) cannot be forgotten. Therefore, when alive, she had publicly manifested her position on both questions. Of course, Cleveland does not even attempt to prove or explain why Mrs. Bent would have wished to repeat herself, clandestinely this time, in an act which would demand much material effort and trouble and would sooner or later stigmatize her and his names for

over.

Ad [D] – This is another erroneous oversimplification of the historical facts: cf. already above, p. 148.

Ad [F] and [H] – Cleveland's scheme is manifestly absurd. According to his scenario, the fraud would have involved several workers to dig a deep hole ... just to plant a fragmentary stamp at the bottom of the pit!!! The all-important testimony of the archaeologist Kelso is reproduced here (cf. BASOR, No. 199, p. 65):

> " Since the digging here presented numerous problems, I personally took charge of the small group of workmen on the site. I was the best person to recognize this artifact. The workmen apparently assumed that it was simply a large sherd for no one asked to see it, although these men always asked me to explain to them the use of anything new that they found. Therefore, since no workman asked for baksheesh although I was paying high baksheesh at Bethel, the seal was definitely not " planted."
>
> Although the seal was found in mixed debris and therefore cannot be dated by context, it was well below the undisturbed surface-level above it. Therefore since the find was not in surface debris where someone might have lost it in the last one hundred years, it seems reasonable to assume that it was either lost or deliberately abandoned by some incense caravan, probably in the ninth century B.C." (the underlining is mine).

Never once does Cleveland's paper openly deals with Kelso's testimony; all that he does is contained in the short incidental phrase "supported by its excavator" (cf. above, p. 136). That is all!!! Yet, Kelso's testimony could hardly be clearer, so is Cleveland's attitude. In order to promote his fantastic thesis, Cleveland completely disregards and distorts the depositions of his fellow archaeologist, to which he does not even condescend a single word of acknowledgment, not even out of consideration for his reader.

Ad [G] – Cleveland's evaluation of his own scheme may be intended to secure his legal liability against any charge of slander. As far as I am concerned, the legal aspect is of no importance. What is important is that the thought of a fraudulent act was planted publicly in the mind of the reader. The author's acknowledgment of his scheme as a "hypothetical model" neither can nor will ever erase the shame incurred by the voluntary attempt to so unjustly smirch Mrs. Bent's memory.

9 – The last paragraph of Cleveland's paper is presented as a "summary" of his scheme. It reads as follows:

> [A] "In summary, the clay stamp of South Arabian origin found at Beitin is
> [B] puzzling. What can now be stated is that Mabel Virginia Anna Bent, known publicly as Mrs. Theodore Bent, had not only the opportunity,
> [C] but a strong motive for visiting the site of Biblical Bethel. Circumstances suggest that she may possibly have had a personal psychological reason for leaving some special possession associated with her late husband's
> [D] achievements interred there. The question which cannot be answered with certainty is whether these new clues to the mystery do in fact provide the real explanation for the clay stamp's discovery at Beitin."

Ad [A] – What is more than "puzzling" in the case of the Bethel stamp is Cleveland's bewildering saga and especially (cf. already above, p. 135) what I called the "deeper reason behind that preconceived theory" because "such a systematic distortion of the written word" is in itself unexplainable.

Ad [B] – That Mrs. Bent had the opportunity of visiting Bethel is not in doubt since she spent an undetermined length of time in Jerusalem, and Bethel is only about 16 klm north of Jerusalem. But, there is not the slightest clue in her booklet that she ever went there. Even if Mrs. Bent did go to Bethel, such a trip would still prove absolutely nothing. Finally, the so-called "strong motive for visiting" Bethel is a pure speculation which is not even remotely suggested by any fact reported in the booklet.

Ad [C] – Those so-called "circumstances" remain not only unknown but also unsubstantiated by her booklet and especially by her mention of Bethel.

Ad [D] – How could Cleveland honestly hope that his scheme "provide the real explanation for the clay stamp's discovery at Beitin" when every single main section of his theory proves to be a distortion of the written word of Mrs. Bent and/or Kelso or a pure invention?

Summarily, Cleveland's theory is so grossly distorting and embroidering the reality as known through archaeology and Mrs. Bent's writings and especially her booklet Anglo-Saxons that it is unexplainable without assuming an ulterior motive so blinding that even the simplest and easily understandable published word is perverted, misconstrued or simply eliminated. Mrs. Bent's memory was and remains unblemished although she was under the lash of a vicious man-made storm which, like a thief, even foraged in her private affective life. Such a storm will be remembered as an irresponsible folly. The Bethel stamp (Artifact No. 2295, unearthed on Monday, August 26, 1957, by James L. Kelso, director of the archaeological excavation by the Pittsburg-Xenia Theological Seminary at Bethel) is an authentic item of Palestinian archaeology.

VIII – A.J. Jobling's epigraphical work.

In the last six years, A.J. Jobling has released several papers which intersect each other on his surveys of the 'Aqaba-Ma'an region, and he regularly publishes Tham inscriptions in a most dilettantish and incompetent manner. He apparently built up his own chart of Tham letters from E. Littmann, F.V. Winnett, etc, and with the help of HaIC, he decided that he k n e w Tham well enough to publish the texts himself. Recently, however, probably in 1984, he revised his chart on the basis of E.A. Knauf's theories (cf. below, D) and he recidivated probably in 1985 for two more letters at F.V. Winnett's suggestion in favor of C. King's theories (cf. below, H). As could have been expected, the result is pitiful. Of course, many of his texts are short and clearly engraved, but even with such easy texts and especially when there is the slightest difficulty of any kind, his incompetence is there for anyone to see. Furthermore, his facsimiles also show a superficiality of examen which matches that of his epigraphical knowledge; his reasoning and argumenting method is askew: the reader very often has the impression that Jobling does not exactly understand the value of the high-sounding words or expressions which he revels in using and that he primarily follows fleeting whims regardless of his epigraphical documentation which is limited to a few publications.

JaMAR, XIV, pp. 123-124 already republished 4 texts from Wādī Ḥafīr which Jobling had presented in JoMoFSA, pp. 400-401. A good photo of JaT 92 is published in JoAMA, II, p. 45.

A – Jobling's antinomic self-appraisals.

It is indeed a very rare event to find an author describing himself and doing it twice in the same year, and it is even rarer when the two presentations contain an apparent antinomy.

It is in the light of the many following pages that the reader may evaluate Jobling's self-assertion which is printed in an official publication of the Australian Government, viz. Australian Foreign Affairs Record, 55, no. 5, May, 1984, where his short communication entitled "The 'Aqaba-Ma'an archaeological and epigraphic survey" (pp. 434-435B) contains the following statement:

> "the main focus of interest and particular area of expertise of the director [= Jobling] is in: early Semitic linguistics and epigraphy" (p. 435B).

Such forthright and unequivocal a statement is unintelligible. Indeed, more than six months after the date of its release in "May 1984" and, to be exact, in a letter dated of December 4, 1984, Jobling himself took the initiative of acknowledging that "I still have a long way to go in North Arabian studies." This statement also is crystal clear. How is it, then, possible to understand that the author's boasting of his so-called "particular area of expertise" (my underlining) in "early Semitic linguistics and epigraphy" of May, 1984, could be justified by, or reconciled with, his later spontaneous avowal of lack of expertise in December of the same year? Also how is it possible to understand that the author could still swamp bulletins with his papers on Tham after and in spite of such equally unambiguous admission of being unexpert?

Furthermore, how can Jobling claim having the "particular area of expertise ... [in] early Semitic linguistics and epigraphy" when, in a paper published the very same year, JoFS, p. 195, footnote 5, he states that

> "The director is obliged to the Abbé J. Starcky for his advice and guidance with this inscription," viz. a short Nabataean

text composed of four words forming the well-known formula ʾlm X bn Y? Starcky was already thanked two years earlier in JoAMS, p. 203, footnote 8, because he "has kindly assisted me with the transliteration and dating of this Nabataean inscription." Moreover, the publication of the first Nabataean text is also printed almost verbatim in JoFSA, pp. 424-425, but no gratitude is here extended to Starcky. One of the rare additions found in this paper (p. 424) is "ADAJ, XIV, 1982, p. 203," where the volume number must be corrected to "XXVI.

Jobling's dependency on Starcky in Nabataean is paralleled by the "guidance of Mr. Michael Macdonald and consultation with Professor F. V. Winnett" in Tham, as he himself states in JoAM, p. 467, whose text is reproduced below, F, introduction. These "guidance" and "consultation" were not at all of minimal importance or on petty details. According to Jobling himself (l.c.), they "enabled a preliminary publication of some of this new material," viz. without them, there would not even have been any "preliminary publication."

Then, how could an author vaunt his own "expertise..." while still in dire need of "guidance," "assistance" and/or "consultation" to the extent that without them he could not even produce "a preliminary publication"? Such a sitation is precisely the opposite of "expertise," which means "specialized skill or technical

knowledge." Jobling's papers prove that he does not have that expertise.

The preceding examples, however, provide a possible (?) way of understanding the aforesaid Jobling's boasting because they make it conceivable that for him "the main focus of interest" e q u a l s "particular area of expertise," viz. the two expressions mean the very same thing, and "and" between the two of them in fact stands for the sign of equality or a comma introducing a mere apposition. This interpretation would justify the singular "is." If so, his statement would be clear and correct, but it would still not justify his publications of Tham texts. The only trouble is that, if such is the case, Jobling should publish his own dictionary and his own grammar. He should do it anyhow because his statement, as it stands compared with the quality of his papers, indicates that he gives "expertise" a meaning foreign to that of any dictionary.

The preceding section A, first half, and C were sent to Religious Traditions last July 16, 1988; the original text, however, has been thoroughly revised and greatly expanded.

B - The beginning of Jobling's epigraphical work.

a - In his report of the first campaign in ADAJ, 25 (1981), pp. 105-112, Jobling simply mentions the existence of some Tham texts in p. 108B when dealing with "Rock Carvings;" no photo, no place name, no detail are given; nor does the caption of the illegible photo of p. 391A give any information.

b - In the report of the second season in ADAJ, 26 (1982), he first refers to the Tham text with the "Reg. No. J14202" found during the preceding campaign at "Sid El Rumi" (p. 199B). The preceding information is repeated and the name becomes "Sad al-Rumi" in p.202A. (These two transliterations of the same name are the first example of one Jobling's trademark). This place name is not to be found in the sketchy map of p. 200. This finding was not mentioned in the preceding report, but the reader finds there the place name as "Sad el Romi" (p. 107C)!!! Then, he speaks of "the example from Wadi Rumman" (p. 202B, and in the caption of the excellent photo of p. 480B) although the first report (p. 107C) makes it clear that Wādī Rummān covers a large area including three jebels and many names of individual places; and the photo siglum is not given. This is the 1st text published by Jobling (cf. footnote 6 of p. 202B):

"6. This inscription may be transliterated: *lrdbntm*."

JaT 127

Whether there is an uncorrected printing error or a mistake in the manuscript is immaterial to the fact that the third letter is ḍ, not "d." However, Jobling would not ratify his decipherment any longer since he changed his [illegible] value of the letter in JoNAT (cf. below, H). Furthermore, it is simplistically [illegible]ish to isolate a bn found toward the beginning of an inscription without even considering the possibility of bn being the part of a name. The text, JaT 127 ([illegible] preceding page for my tracing) may be read as follows:

lrḍbn tm Ry Raḍbān. He has loved excessively.

rḍbn, cf. Saf rḍwb of WH 679, and Ar raḍaba "to suck, draw in with one's lips (a young woman's) saliva" (cf. LaAEL, p. 1096A). - tm, e.g., JaT 91.

3 - With his two reports of the third campaign, Jobing proceeded in earnest to publish Tham texts.

Here follows the detailed study of six of Jobling's papers. That they were not taken chronologically does not matter at all because there is no difference in the quality of the work whether the article was written in 1982 or in 1988.

C - In JoDD (cf. Religious Traditions, 7-9 [1984-1986], pp. 25-40), Jobling aimed at proving the existence of the god ḏšry = ḏšr = ḏšr in Tham inscriptions and also at illustrating the well-known mention of the goddess Lat in the same texts.

1 - Preliminary remarks. - The origin of JoDD 1, 3 and 4, and the location of the places where JoDD 5 and 6 were found remain unknown; and JoDD 2 comes from Wādī Ḥafīr. No. 5 comes from "the upper reaches of the Wadi edh-Dhiqa" (p. 30). Is it the name of the place "Dhega" which belongs to "Wadi Dhiqa" according to ADAJ, 25 (1981), pp. 107B and 110? At any rate, both "W. Dhiqa" and "Wadi edh-Dhiqa" are used in JoFS, pp. 191B and 197A, 198A, 200A, respectively. The Ar spelling of the name remains unknown. — For Jobling's various transliterations of the name Ḥafīr, cf. below D; cf. already above, p. 162, the same inconsistency in "Sid El Rumi." — The absence of photographic documentation prevents the reader from checking the author's facsimiles against photos; thus, the reading of his texts cannot at this time be ascertained; this is the reason why those texts are not given JaT sigla in the following study. — The photo of JoDD 5, however, was published in JoFS, p. 470A, and the text, JoFS 16, is given on pp. 198B-199B, with the same commentary as here. There are no cross-references in the commentaries of either JoDD 5 or JoFS 16. Furthermore, the siglum of the photo is "AM84/52/36" in JoDD (p. 30), but "AM84/52/MB" in JoFS (pp. 198A and 470A). What is the justification for inventing a new siglum? "MB" stands for "Michel Bannigan," who, being responsible for photography" (p. 191A), took all of them any-

way. — The author's facsimiles printed in other papers are approximative, at best. It is out of necessity that the published facsimiles are retained here; they are reproduced below in the position indicated by their scale. Jobling fails to justify why he set JoDD 2 on its left side, and both JoDD 5 and 4 on their right side.

Jobling's facsimiles of JoDD 1-7

Most pages of the paper either contain data already known or are superfluous, viz. the expose of the self-evident sections of JoDD 5-7 (pp. 31-32, 34-35 and 35-36) with the useless duplication of transliterations and translations, or the short sketch of the well-known etymologies of lt (pp. 33-34). Also quite unacceptable is the quality of the translations and of the few argumentations directly relating to the texts. — Finally, the author's translations of JoDD 1-3 are in the line of F.V. Winnett's and especially in Berytus, 22 (1973), pp. 64-89 and 96-100. Jobling lists this paper in p. 39, footnote 5, with two errors in the title, viz. "archaeological and epigraphical" instead of "archaeological-epigraphical," and "Hā'il" instead of "Ḫā'il." For my first installment of the Ḫā'il Tham texts published by F.V. Winnett in the first section of the aforesaid paper, cf. JaMAR, XIII, pp. 151-176.

2 - Decipherment. - Beside his usual alternating of two letters of 1/8 and 3/1 as "Z" of which the actual form is a crux immissa, the author reads as "Ṣ" the letter of 1/13 for which he also defends the value of "Ṯ" in JoNAT, cf. JoNAT 4/3 and 13/2 = JaT 115c and 124b, respectively (cf. below, E). — Similarly, he reads the letter of 1/5, 2/5, 3/6, 5/21, 23, as "Ṣ," but also gives the value of both "Ṭ" and "Ṣ" to the same letter in the same JoNAT, viz.

"Ṭ" in JoNAT 1/2 and 15/3 = JaT 112b and 126c, respectively;

"Ṣ" in JoNAT 4/2 = JaT 115b, etc, and

"Ṭ" and "Ṣ" in the same JoNAT 5 = JaT 116 (for all these texts, cf. below, E); cf. already JaMAR, XIV, p. 27.

The author reads the letter b of 5/22 as "R" as in 5/19, although the two letters are completely different, in order to find the divine name "ḎŠR" which he is looking for. The letter of 6/13, 24, 58 and 7/1, 24 is read as "Ṭ" although the letter manifestly is ṣ. In JoDD 6 the author inserts an adventitious dot or speck on the stone as the letter "Ṯ" into the second name after dkrt lt; he eliminates the initial letter of the following name ʿqrb; and finally he misreads the antepenult letter as "Ḏ" instead of r. Summarily, the bivalency given to the letters of 1/5, 2/5 and 1/13, the confusion of the letter of 1/8 with the crux immissa, and that of the letters b and r make any decipherment quite easy and practically allow anyone to find the words of his choice.

3 - The sources. - According to the author himself, eight books "have been used in the study and presentation of this new material" (p. 39, endnote 6). Among them, only one contains the publication of a collection of Tham texts, viz. his no. 2, which is HaLiSTI (1952), and his nos. 3 and 5 are Ar dictionaries. What right does the author have to exclude all other publications on Tham, which surely are more relevant to Tham than an Hebrew dictionary (his no. 4) or a volume on Nabataean (his no.6)?

4 - The word wdᶜ of JoDD 1 and 2.

a - What is the meaning of the initial phrase of his commentary on JoDD 1, viz." The form WDᶜ is uncertain " (p. 27). The two immediately following phrases deal exclusively with etymology and do not contain a single word on the kind of part of speech the word actually is, viz. noun or verb.

[B] [A] b - The author states that " The root means either "to be gentle, still or calm", or "to deposit (something with someone for safekeeping)" " (p. 27).

Ad [A] – This is the meaning of Ar wadu'a. The first adjective is the initial meaning given by H. Wehr – J.M. Cowan's dictionary (p. 1059B), which is not mentioned in his list of eight books used for his paper (cf. above). The expression "still and calm" comes from p. 32 of HaLiSTI, the no. 2 of his list. This information is very important; cf. below, 6.

Ad [B] – The second meaning of wd', viz. "to deposit (something with someone for safekeeping)," which belongs to Ar wada'a, is a distortion of J.G. Hava's dictionary, the no. 5 of his list, which gives "To deposit (clothes) in (a ward-robe)" (p. 859B). Therefore, in order to render the meaning applicable to his translation, the author stretches the word "clothes" to the extent that it becomes "something" or anything, and metamorphoses a place (="a ward-robe") into a human being (= "someone"). For another example of a similar distortion, cf. below, 7b.

c – His reference of wd' to "D'W" is preposterous. Besides, the initial w would then become the conjunction "and" which is not accounted for in his translation.

5 – The divine name "DŠHY" of JoDD 1 and 2 = "DŠR" of no. 3.

Jobling translates the divine name of JoDD 1 and 2 as "Dū Sārā" [sic!!] (p. 27) and "Dushares" (p. 28; cf. also p. 29 in JoDD 3: "DŠR") without justifying the use of an Aramaic form in the translation of a Thamudic inscription (cf. below, and of 6).

6 – The formula "WD' DŠRY" of JaDD 1 and 2.

Jobling copied the last two-thirds of his first meaning of wd' from HaLiSTI, as stated above (cf. 4b) and more precisely from the commentary on HaLi 299. But, the beginning of this text also has "wd' dary," exactly as in JaDD 1 and 2. Therefore, from Jobling's viewpoint, this information contradicts his claim that his JoDD 1 "is important in that it introduces us to popular devotions to Dusharas" (p. 27; the underlining is mine). Furthermore, how is it possible to copy wd' "means 'to be still' or 'calm'" in two lines and not see "wd' dary" in the line immediately above? Similarly (cf. below, 8), Jobling also copied from p. 53A of the same HaLiSTI his references to the HaLi texts mentioning dkrt. Here again, how is it possible to copy those numbers without seeing "dar Dusharas 299" in the line immediately above? Moreover, the palaeography of HaLi 299 is identical with that of JoDD 1; thus, they both have the same person as author. Note that the republication of HaL 299 printed five years ago in JaMAR, XXII, p. 238, which is not referred to by Jobling, does not mention any divine name. Finally, it is also from

the commentary on HaLi 299 that Jobling copied "Dusharas" while omitting either "r" after "D" or to underline the same "D."

7 – The pericope "WD' DŠRY X" of JoDD 1 and 2.

a – X is "ZMLT" in JoDD 1, and "SR" in JoDD 2. Although the formula is identical in both texts, Jobling takes X of the first text as a noun, but as a personal name in the second. Not only he does not justify his choice but he does not even mention the alternative possibility, nor does he consider the feasibility of taking both words as nouns or names.

b – Jobling recidivates his falsification of the written word (cf. above, p. 166) in the translation of his "ZMLT" (no. 1) as "companionship" (p. 27). The only Ar dictionary listed in his endnote 6 which mentions "zmlt" is Hava where the reader finds this: "zumlat Party, companions" (p. 296B). Here again, in order to render the meaning applicable to his translation, the author atomizes the group of human beings referred to by "Party, companions" so thoroughly that they become an abstract idea, viz. "companionship."

8 – The verb dkr of JoDD 5, 6 and 7.

The verb dkr to remember is translated in four different ways by the author, viz. "remember" in JoDD 5 (pp. 30 and 31);

"bless (remember for good)" in JoDD 6 (pp. 33 and 34); and

"bless" and

"remember (favourably)," both in the same JoDD 7 (pp. 35 and 36, respectively).

In his commentary on JoDD 5, the author notes that "whether such remembrance is for good or ill we are not told in this inscription" (p. 31).

His question is irrealistic because there cannot be any doubt that the meaning of the petition was beneficial since the persons are not presented as adversaries. Then, the author adds that in the HaLi texts "the wellbeing or benefit of the person, or persons, mentioned may have been inferred" (p. 31) with reference to endnote 10 (p. 40) which reads as follows:

"10. G.L. Harding, op.cit., 1952, pp.45, 54, 156, 178, 481, 489, 506, 526, 821."

Here, "pp." (= pages) must be corrected to "nos," and the numbers are exactly those given by HaLiSTI, p. 53A. Furthermore, Jobling's so-called inference is erroneous because each of the HaLi texts contains nothing more, nothing less than the very same formula of JoDD 5, 6 and 7, viz. dkrt lt + at least one personal name. Moreover, if the author's inference is incorporated in his translation of JoDD 6 and 7, why is it not in his translation of JoDD 5? It should also be added

that the root ḏkr does not mean "to bless." Finally, the self-evident propitiatory meaning of the verb ḏkr is proven apodictically by JoDD 6, but Jobling does not see what is staring him in the face, viz. ʾšsy, the grandfather of bnt, the author of the text, is the first direct complement of ḏkrt, and bnt states that he himself wrote the text; therefore, bnt requests lt to remember his own grandfather. Incidentally, in bnt dʾšsy at the end of the text, d does not introduce the name of a "tribe," as Jobling states twice in his translations (pp. 33 and 34) although he himself speaks also twice of "family or tribe" is his commentary (p.35); it obviously introduces the name of the family founded by bnt's grandfather and quite naturally named after him. A final note on the same JoDD 6 further illustrates the author's inconsistency. The end of the text,

" W. BNT. KTT D. TṢY" (p. 33) = " W. BNT. KTT. D. TṢY " (p. 35) is translated in

two different ways, viz.

" and BNT wrote (inscribed) [sic!] this of the tribe of TṢY" (p. 33), and

"and BNT inscribed (it) of the tribe of TṢY" (p. 35).

It is difficult to figure out the reason which prompted Jobling to insert his "(inscribed") after "wrote" in his first translation as if nobody would understand "wrote," since he does not insert it after "wrote" in JoDD 1 (p. 27) and 5 (p. 31). Furthermore, "this" = "(it)," although neither exists in the original, is not followed by a comma. Finally, "the tribe," which as such does not exist in the text, must at least be put between parentheses.

9 – The phraseology of Jobling's translations of JoDD 1 and 3.

His translations of JoDD 1 and 3 read as follows:

"prayer to Dū Šārā (is for) companionship, and ĞR wrote (it) " (no. 1, p. 27); and

"Fructify, O Dushares, ḤTY. ʿBD inscribed (this prayer) " (no.3, p. 29).

All parentheses must be corrected to brackets, and "for" of JoDD 1 and "O" of no. 3 must also be put between brackets because all the words involved are inexistent in the original texts. In the first translation, the restoration of not only one, but of two prepositions after a noun is unjustifiable. Moreover, a phrasaeology beginning with a word, such as "prayer," does not exist in Tham. In the second translation, the use of the verb "fructify" is very awkward, to say the least, in connection with a human being. Jobling necessarily understand his "ḤTY" as a person since he does not note that it might be something else. Furthermore, "(this prayer)" is the author's summary of his overall interpretation of the text and, thus, belongs to a commentary, not to a translation. Finally, the author invents the invocative particle h– before the divine name. Were the text an invocation, the particle h– would normally have been engraved.

10 – Jobling's theophoric name "ʿḎ LŠRY" of JoDD 4.

His admission of the name as theophoric disproves its division into two sections; yet, he does it in both his transliteration and translation (p. 29). His commentary (p. 30) does not even consider the possibility of his "ḎŠRY" being an invocation similar to that which he found in JoDD 3 and, therefore, with the following translation: "By ʿḎ, O Dushares." Furthermore, he does not give a single reference to his name. That he does not have any is proven by the fact that he refers to "the wide range of similar North Arabian theophoric names of the pattern: ʿḎ + X" (p. 30); and he lists as examples ʿbdʾl, "ʿḎ LḪ" [sic!], ʿbdšmšm, ʿbdʿttr and ʿbdlt" with reference in endnote 9 (p. 40) to HaAIC, "pp. 396-397." First, the author does not explain why he transcribes "ʿḎ LḪ" in two sections, but "ʿḎLT" in one. Secondly and obviously enough, his list of five names only is a very sketchy presentation of the ʿbd+divine name nominal formation. Furthermore, since he refers to "pp. 396-397" only, he did not check the two SA names ʿbdʿttr and ʿbdšmšm on p. 399. Unfortunately, the general affirmation of his quotation is erroneous because that p. 399 does not list a single NA reference under either name. The preceding fact and all previous remarks prove that the author is mostly, if not exclusively, interested in his own ideas and not in the actual documentation, and also that it is on the basis of the former that he understands and presents the latter; viz. subjectivity replaces objectivity. Finally, Jobling still is unaware of the existence of Tham theophoric name ʿb[d]dr of Hu 501/1-2, which was republished 14 years ago in JaMAR, V, p. 81.

11 – Jobling's commentary on JoDD 5.

Jobling's commentary on JoDD 5 contains the following remarks (p. 31):

" This is an interesting inscription because of its length, content and association with other inscriptions on the same panel. It is possible that it is composed of two inscriptions, the prayer to DŠR being added later. This may explain the logic of the final phrase: W ḤBB KTT: and ḤBB wrote (it). "

These remarks are repeated in p. 32 in the same commentary on no. 5 as follows:

" As noted above its present position and the variations in the style and execution of the script may suggest that the prayer to which it is appended was drawn some time after the inscribing of the first phrase. "

Here again, Jobling's inconsistency is manifest and ridiculous. He does not make

those remarks on JoDD 6 which is even more elaborate than JoDD 5. Furthermore, how is the reader of his paper supposed to check his reasoning based on the "association with other inscriptions of the same panel" since Jobling does not give him a photo to look at and does not even refer him to the other bulletin where he published it? But, when the reader looks at the photo published in JoFS, p.47CA (cf. already above, pp. 163-164), he is flabbergasted by the emptiness of Jobling's remark. Moreover, what does he mean by "the variations in the style"? As far as I am concerned, they are empty words. Finally, Jobling's suggestion of the two texts also is ridiculous based, as it is, on nothing factual but only on a fleeting whim. There is no difference at all in the ductus of the letters, and the position of the text on the panel most certainly does not even remotely suggest his fancy idea.

12 - The end of JoDD 6.

The end of JoDD 6 is characterized by the insertion of a verb (here, ḥṭṭ) inside the author's identity card; the same feature is also found in both JaT 101 (cf. below, D) and 105e (cf. below, E), viz.

JoDD 6:	bnt ḥṭṭ		d³šqy
JaT 101:	(2) ḥtt ḥṭ(3)ṭ kll	(4) bn btl	(5) t
JaT 105e:	(1) ṭrt ḥṭṭ	(2) bn zd(mt?)	

Jobling does not make any remark on the special feature of any of those three inscriptions; and in the last two instances his transliteration restores the normal order of the words and ascribes them to a single line. The preceding feature is also found in Ṣa³il 122a; cf. JaMAR, XIII, pp. 167-168, where parallels are cited.

13 - The texts.

JoDD 1 may be read as follows: wd⁶ dlby dm lt wgr ḥṭṭ

Wādi⁶, he of Labîy, has blamed Lât; and Gâr has cut [this].

The decipherment of sections 1, 5 and 6 is as in the edition. - wd⁶ dlby, already in HaLi 299; dlby, also in JaT 126c, and lby in JaT 118b. - dm: the passive voice is attested in Hu 12. - lt is also found as a personal name in Saf, e.g., JaS 170. - gr, e.g., JaT 106i and 115c. - ḥṭṭ, e.g., JaT 101.

JoDD 2 may be read as follows: wd⁶ dlby sr Wādi⁶, he of Labîy, has travelled.

The decipherment of sections 1 and 3 of the text which was engraved by the author of JoDD 1, is as in the edition. - sr is well-known in Saf, e.g., WH 302a.

JoDD 3 may be read as follows: dr⁶ dlrḥ ty ⁶bd ḥṭṭ

Dâri⁶, he of Hlrawḥ, has died; ⁶Abd has cut [this].

The decipherment of sections 4 and 5 is as in the edition. - ⁴r⁶, e.g., JaT 333. - lrḥ, a l(= il)-qtl nominal derivation; cf. rḥ of HaLi 156a. - ⁴y, e.g., CIS 2076. - ⁶bd, e.g., JaT 33c. - The text is an announcement of death.

JoDD 4 may be read as follows: l⁶bd dlby By ⁶Abd, him of Labîy.

The decipherment of section 1 is as in the edition. - This ⁶bd, who was a member of the family lby, as wd⁶ of JoDD 1 and 2, is not to be identified with his homonym of JoDD 3 because the ductus of b is opposite to that of JoDD 3.

JoDD 5 may be read as follows:

lḥbb bn bnlh bn ḥbb wḏkr dlb lkmlh wḫbb ḥṭṭ

By Ḥabîb, son of Binlah, son of Ḥabîb; and Lâlib has remembered Lakimlah; and Ḥabîb has cut [this].

The decipherment of sections 1-6 and 9-10 is as in the edition. - ḥbb, e.g., Ṣa³il 18. - bnlh, a theophoric name composed of bn+lh. In HaAIC, p. 171, the reference to Saf LP "146" must be corrected to "1146," where the name reads rnlh (cf. JaSN, p. 107, no. 8). Also the reference to Saf "WH 584" is erroneous; the name is bnkh (cf. JaMAR, XIII, p. 201). Finally, the reference to HaLi 483, which considers as certain E. Littmann's doubtful meaning, "bnl(h)" which should have been printed "bnl[h]," is unacceptable; cf. JaMAR, XIV, p. 26. - dlb, cf. Saf CIS 3380. - lkmlh, cf. Tham lkml of Hu 307/2.

JoDD 6 may be read as follows:

lbnt bn knḍ bn ³šqy wḏkrt lt ³šqy w⁶lmh w⁶qrb w⁶wḍ wl⁶n kllhm wbnt ḥṭṭ d³šqy

By Bânit, son of Kânid, son of ³Ašqay; and may Lat remember ³Ašqay and ⁶Almuh and ⁶Aqrab and ⁶Awad and La⁶in, all of them; and Bânit, he of ³Ašqay, has cut [this].

The decipherment of sections 1-4, 6, 7, 11 and 13-16 is as in the edition. - bnt, e.g., Ṣa³il 184B. The name bnt is related by HaAIC, p. 119, to the root "bn; Ar banna fragrant." But, Ar bannat is not an adjective, but a noun, viz. "odor, pleasant or unpleasant." This etymology remains unjustified because bnt would then be a qtl+t derivation. The name is a qtl formation derived from Ar banata, 2nd form "rogavit, multum sciscitatus fuit" (cf. FrLAL, I, p. 159A). - knḍ: Jobling considers the initial letter as doubtful, "ḴNḌ" (pp. 33 and 34). The 3 1/3 lines of commentary (p. 34) contains nothing worth reading, but not a single word is given to justify the small circle above the initial letter; for this letter, cf. below, D III on JaT 102a). Cf. Ar kannaṣa "to turn up one's nose" (cf. Wörterbuch der klassischen arabischen Sprache, I, Wiesbaden, 1970, p. 387B). - ³šqy, cf. Ar šqw

ś̌ʿy, and ś̌ʿyw "haroshíp" (cf. LaLEL, p. 1548A-B); cf. also in Saf ś̌ʿw of CIS 4204 and ś̌ʿwʾ of Amman 2; the noun ś̌ʿy wickedness is well-known in Sa (cf. JaSIMB, p. 449B, and Cahiers de Byrsa, 8 [1958-1959], p. 167). – ʿlmh: Jobling's reading of "ʿLYMH" is inexplicable; cf. ʿlm of HuIR 139. – ʿgrb, e.g., Hu 421a. – ʿwd, cf. Saf, e.g., Jap 42, and Tham ʿwdlh, e.g., in JaT 106j. – ʿṯn, cf. the verb to reject, e.g., in Hu 627.

JoDD 7 may be read as follows: lʾs̆ʿy bn whblh wḏkrt lt mnʿ wʾʾs̆ʿy wnṣrlh

By Sa'say, son of Wahablah; and may Lat remember Manuʿ and Ass'say and Nasarlah.

The decipherment of sections 2-6 is as in the edition. – ś̌ʿy, cf. ʾś̌ʿy of JoDD 6 and commentary. This person most probably was a cousin of bnt of JoDD 6 and another grandson of ʾś̌ʿy who is mentioned in the petition to the goddess. – whblh, e.g., HaLi 37. – mnʿ, cf. Saf, e.g., Irbid 1812a, and the Tham verb, e.g., of Hu 315/2. – nṣrlh, cf. Saf nṣrʾl, e.g., of JaS 35, and the Tham verb nṣr, e.g., of JaT 12/2.

The imprecation to the goddess Lat is well-known; cf., e.g., Madaba 507. But the texts published by Jobling do not contain the divine name d̆ṣr = d̆ṣr = d̆ṣry. It is regrettable that the author would deal with Tham texts for which he manifestly remains unprepared.

D – JoPRS (cf. ADAJ, 29 [1985], pp. 211-219 and pl. 43-50).

Jobling studies four Tham inscriptions from Wādī Ḥafīr (about 78 klm northeast of ʿAqaba in crow-flight distance), which his team explored during the winter months of 1984-1985.

1 – Introductory remarks.

a – Jobling gives the Ar name as وادى صفير, and the "English name" as "Wadi Hafîr" in JoPRP, p. 199 (the sketchy map in p. 198 is illegible), but also as "Wadi Hafir" in p. 205B. Jobling certainly has the right to transliterate the Ar names as he wishes; but the system adopted should be adequate and consistent so as to make the Ar names easily recognizable. Here follow a few examples of Jobling's spellings of Ar names (p. 199):

بير الفولا "Bir al Fuweli;"
ابو الضبعة "Abu Al Daba';"
جبل امزيقة "Jebel 'Imzêqua;"
وادى ظرافة "Wadi Darafah;" and
جبل ام غدة "Jebel Um Ghadah."

The uniform transliteration of Ar ذ, ض and ظ as "d" and the confusion of ه and ح and of ء and ʿ are conspicuous. Such spellings remind the reader of H.St. J. Philby's extravaganzas; cf. already above, p. 162, for the case of "Wadi Al Rumi." Furthermore, in the present paper, Jobling maintains one of his 1985 spellings of "Wadi Hafir" (p. 213, map, and also in Syria, 35 [1985], pp. 322 and 323A) and adopts a new one, viz. "Wadi Ḥafir" (pp. 212A, 213, 214B, 215B, 217B and 219). Then, in JoAMA, II, repeats "Hafir" (p. 46); JoAMA, III, also repeats "Hafir" (p. 234), changes once more to "Ḥafir" (pp. 235, 236 and 239-245) and, finally, uses both "Hafir" and "Ḥafir" in pp. 238 and 239. Note also that the "Map 'Aqaba-Ma'an Survey 1985" of JoPRS, p. 213, is also printed in JoAMA, III, p. 234, where it became the "1985 Preliminary Survey Map." In other words, Jobling's publications, which often duplicate or overlap one another, can hardly serve as models of lucidity and consistency.

After listing some geographical names in JoPRS, Jobling publishes the following note (p. 201):

> "(Note: The Grid References in this table are based on the Archaeological Map of the H.K. of Jordan 1:250,000 of 1982. Standardisation of English and Arabic names still remains a problem. English spellings from old maps have been retained for the present purposes.)"

The problem of "standardisation" [sic!] which each author has to solve is a lame excuse which cannot justify his inconsistent looseness or carelessness in reproducing those names; a problem is not solved by messing it up. Is it so difficult to adopt a rational spelling and to maintain it? Apparently, it is an impossible job for Jobling.

b – The present Tham photographic documentation is important from a methodological viewpoint. The new epigraphical material was selected not to give a general idea of the contents of the inscriptions but to repeat parrot-fashion one of E.A. Knauf's concoctions, viz. the letter ṯ has the value of "g." The first photo comes from Jobling's fourth season (cf. JoPRP, pp.205-206 and pl. 44B-45B), and the third also "was initially recorded in 1983" (p.215B).

Once his selection of the photos was completed, Jobling proceeded with yet another selection: he retained for his study what he thought to be related to his thesis; therefore, the balance of the photos was simply discarded as if inexistent, and there is no trace of it in both his facsimiles and commentaries. The eliminated sections of his photos are as follows:

five human and animal drawings, viz. JaT 102 U, V, 103 I and 104B, H, and

eleven texts, viz. JaT 103 a, c-i, and 104 a, c and c.

Therefore, the actual situation, which is hard to believe, is this: although incompetent in Tham studies, Jobling devotes one of his archaeological reports not to prove, but to smugly propagate a theory on the onomastic value of some Tham letters which, for obvious personal advantages, he copied from E.A. Knauf and swallowed as spring water, and he does it by shamelessly mutilating his own photographic documentation.

The photographic documentation on epigraphy consists of four plates only, viz. pl. 43-46 (pp. 341-344) with JoPRS 1, 3, 4 and 2, respectively. — The photo of JoPRS 2 is rotated 90° counterclockwise. — In the single p. 215, there are three errors of reference to the photos; viz.

on JoPRS 2, "Pl. XLIV" (p. 215A) must be corrected to pl. 46;
on JoPRS 3, "Pl. XLVI" (p. 215B) must be corrected to pl. 44; and
on JoPRS 4, "Pl. XLVII" (p. 215B) must be corrected to pl. 45.

c - Joblings's facsimiles are approximations only, and their main value is negative, viz. they illustrate what should never be done.

d - In my tracings, a capital letter is given to each item so as to facilitate the reference to any particular section of the photo. When a text is composed of several isolated sections, the capital letter is followed by 1, 2, etc.

Concordance of my tracings (cf. the next four pages) and Jobling's:

JaT 101 = JoPRS 1. - A; B1 + B2 + B3 + B4 + B5 = text = JoPRS 1; C; D; E; F; G.

JaT 102, less U and V, = JoPRS 2. A; B1 + B2 + B3 = text a = JoPRS 2; C; D; E; F; G; H; I; J; K; L = text b; M; N; O; P; Q; R; S; T; U; V.

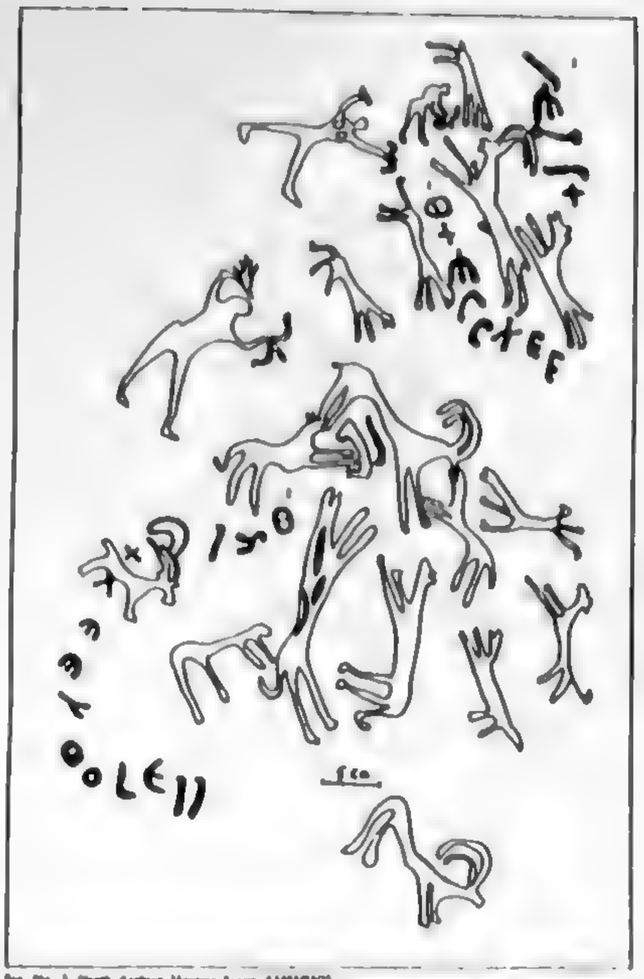
JaT 103, less B, D, E, F, G, H, I, J and K, = JoPRS 3. - A; B = text a; C1 + C2 = text b = JoPRS 3; D = text c; E = text d; F = text e; G = text f; H = text g; I; J1 + J2 + J3 = text h; K = text i.

JaT 104, less A, B, E, F, G and H, = JoPRS 4. - A = text a; B; C1 + C2 = text b = JoPRS 4; D; E = text c; F = text d.

The scale of JaT 101, 102 and 103, 104 is 5 and 10 cm, respectively.

Fig. No. 2. North Arabian Hunting Scene AMIDGRIII (cf. JoPRS, p. 214)

JaT 101 = JoPRS 1



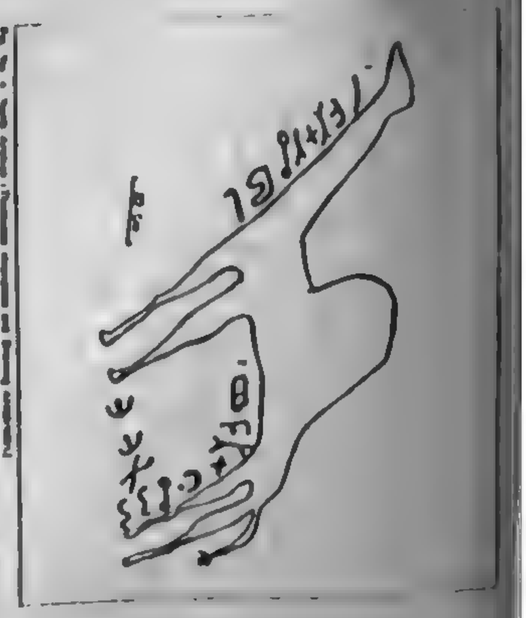
(cf. JoPRS, p. 216)

JaT 102;
cf. JoPRS 2

B1+B2+B3 = text a;
L = text b.

JaT 103;
cf. JoPRS 3

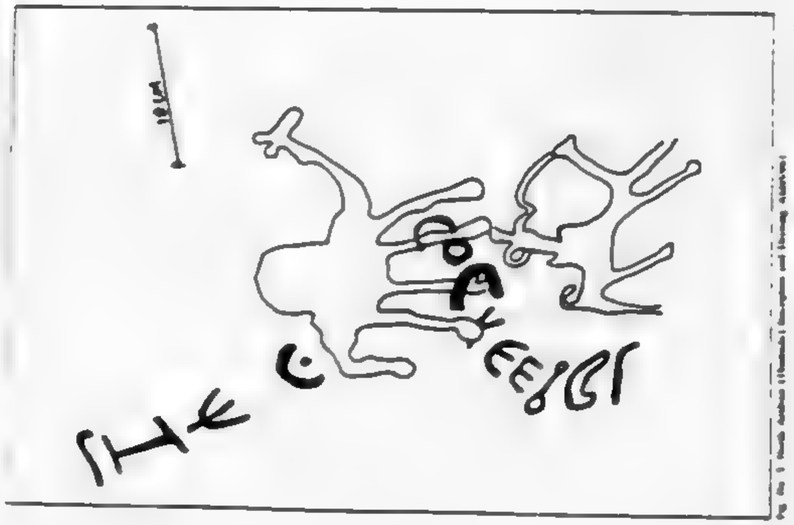
B = text a;
C1+C2 = text b;
D = text c;
E = text d;
F = text e;
G = text f;
H = text g;
J = text h;
K = text i.



(cf. JoPRS, p. 217)

JaT 104; cf. JoPRS 4

A = text a; C1+C2 = text b; E = text c; F = text



(cf. JoPRS, p. 218)

2 - The petroglyths.

a - JaT 101: the photo was already published in JoAMA, II, p. 46. The petroglyths are described by Jobling as a "hunting scene" (p. 214A). It is quite superfluous to spell out such obvious an evidence if no description is added. Jobling does not indicate how many beasts and/or how many hunters are involved. The two hunted ibexes are standing, looking rightward, and they follow each other, the male E in the lead, and the female F behind. Perpendicularly to the forelegs of E and one on each side are two dogs A and G, with gaping muzzles and running at top speed toward the ibex E. Jobling mistook a feature of the stone as some kind of fifth (!) leg of dog G. Standing above F and aslant to the right is D, a bowman with arms outstretched horizontally and with bow and arrow in his hands. A long arrow has already transpierced the hindquarters of ibex E. The only detail difficult to understand is the long line curving to the right and originating from the neck of ibex F. Finally, Jobling's facsimile distorts too many features; e.g., the first letter of the text, to which he gives a figure which might be that of a spinning ballet dancer, and a feature of the stone between the hindlegs of G, which he represents as if there was a blank in the engraving. That feature is similar to those below the tail of the same G, above t of B1, and below and to the left of t of B3. Furthermore, the narrow blanks in the body of the same G and in both one hoof and one ear of E are eliminated. Furthermore, contrary to Jobling's opinion, the panel contains beside the aforesaid hunting scene the rather awkward design of camel C located between B1 and bowman D.

b - JaT 102.

The published photo (p. 344), which is printed on its left side, cut off the tail and adjacing half of the hindquarters of upper dog A. This cropping is all the more unjustifiable in that about 2 cm of the bottom of the photo with no trace of any kind of engraving could easily have been eliminated. - The two upper panels represent two hunting scenes similar to that of JaT 101. - The distortions of Jobling's facsimile are as manifest as they are numerous; e.g., the shape of hunter D, and the initial letter of B3. - According to the author, "this panel may be divided into two sections" (p. 215A).

" This panel may be divided up into two sections. In the upper panel it is interesting to note that the stylized human figure on the left is a woman. The breasts on this figure are quite clearly represented in the rock drawing. Thus the scene captures a hunting episode in which a huntress is

involved. If the scene is to be identified with inscription No. 1, then it is suggested that we not only have here a good clear picture of a female involved in hunting the ibex but that we know her name to have been thbb."

(1) The drawing of that person, as that of several other petroglyths (e.g., ibex J), is characterised by a heavy outlining of the figure. Thus, the two triangular (not elliptical, as in Jobling's facsimile) blanks on the chest have no more value or meaning than that of the blanks of the running dog O; cf. also the blanks of JaT 101 E, G (cf. above, p. 175), JaT 106 W, 117, 118 D, Q, S, and 130). Such blanks, as well as the above-mentioned heavy outlining, are but the consequences of the personal stylistic preferences of the engraver. The interpretation of those of D as representing female breasts is as ridiculous and as "farfetched and even silly" as E. Anati's interpretation of them as "cache-sexe" (cf. JaMAR, I, p. 79). The perfect parallel of D, however, is another bowman's bust divided into two tall, upside-down, blank, isosceles triangles shown in p. 345 of Jobling's paper, extreme left figure. There obviously is no question of a woman in the present case. Another case was published in JoAMS, pl. 56A in p. 479A, which the author describes as

> " the peculiar human figure (Pl. LVI No. 1) which displays an X-ray style of artistic execution " (p. 199B),

but he later labels it in the caption of pl. 56A as " The X-ray Madonna of Abu Sad " !!! Here again, it is bizarre to find a "X-ray Madonna" or "the X-ray style," an expression also used in the description of the paintings by the Gagadju of the Kakadu country, in a linear drawing of a man with three lower members of equal length and the ribs protruding from the rib cage!

(2) Jobling fails to explain his remark according to which "a huntress [in fact, the man D] is involved" (p. 215A), viz. whether or not that person is the only human being involved. In fact, there is a second human being, C, who is represented in a much smaller scale and might, therefore, be a youth, son or servant, related to the adult D.

(3) Jobling also fails to itemize the animals and, therefore, to inform the reader if he considers dogs F and H as belonging to his "upper panel" or not. As in JaT 101, the running male ibex E is represented between two running dogs with muzzles gaping, A and G. But, the two dogs F and H belong to the lower hunting scene because they are below ibex E and are running in the direction opposite to that of the ibex.

Jobling continues saying that

> "In the lower panel, a second hunting episode is recorded in which stylized human and canine forms are portrayed along with the long horned animal which is the object of the hunt as in the top panel."

Here again, Jobling fails to specify how many humans and how many dogs are involved. As a matter of fact, his "lower panel" contains four petroglyths, P, S, U and V (cf. below), which do not belong to the lower hunting scene. This scene is composed of the following elements, viz. archer I aiming at the hindquarters of the stopped male ibex J, and eight dogs of which two, F and H, are running behind the animal; dog K, whose name is L, is standing behind the ibex and grabbing one of its hindlegs, and six other runnig dogs facing the ibex, M, N, O, Q, R and T. The muzzles of the dogs are, here also, gaping except that of Q which is closed.

Below and to the left of the above-described lower hunting scene, there are four isolated figures, viz. the walking ibex P on the far left; the undetermined, also walking, quadruped S to the left of the hindquarters of O; a crude standing man with two clubs perpendicular to his head and one club is held in his left hand, U, which is missing in Jobling's facsimile; and, finally, the tailless ibex V at the bottom. Ibex P was added after the engraving of text E3, and the right half of the letter h was destroyed in the process.

c - JaT 103.

The photo of JaT 103 is that which is the most vandalized by Jobling. Such a flagrant disrespect for an antiquity is the unmistakable proof that all that counts for the author is his thesis, ... and the rest can go to blazes. Although encircled by text C2 on three sides and on the other by one foreleg of camel A, the small camel I does not seem to belong to the complex A + C, no more than B. As already mentioned above, the small camel I and no less than eight inscriptions are simply ignored by Jobling.

d - JaT 104.

The photo of JaT 104 was also greatly pillaged by Jobling; three out of the four texts and two petroglyths are wanting in his facsimile. Furthermore, his drawing of G, a cameleer on his mount, is unjustifiable because that petroglyth, as B, has nothing to do with the text accompanying camel D. H is a crude representation of an ibex.

e - Jobling does neither mention nor explain the two inconsistencies of his facsimiles. The petroglyths of JoPRS 1, except that of C,

(p. 214) are completely inked in so as to represent the stippling engraving of the human and animal bodies. But, in his facsimiles of JoPRS 2, 3 and 4 (pp.216, 217 and 218), the very same stippling engraving is ignored and represented as a blank, the figures being simply delineated. Furthermore, Jobling adds another inconsistency in his facsimile of JoPRS 2. Three of the four blanks of dog 0 are filled with ink, but the two blanks of the man D are simply delineated.

3 - Epigraphy.

Jobling's epigraphical study, which is the most extensive section of his paper, is so poor and shows both such revolting a narrowmindedness and an embryonic knowledge that it would be very difficult to do worse.

a - A novelty of his paper is the insertion of a dot, ., after each section of the text, a novelty which he had introduced already in JoDD. Not a single note justifies this ridiculous innovation as if the blank between the sections was not enough to isolate one section from the other. Then, the reader wonders why those dots do not appear in Jobling's "Arabic." This word does not mean Ar language, but simply the transliteration of the text in Ar letters, viz. what is so special in the transliteration in modern western letters that the same system apparently is unapplicable in the transliteration in Ar letters?

b - Jobling's text involved in the following remarks is reproduced above, pp. 179-180. As a consequence of his erroneous study of JaT 102 D, Jobling takes that man to be "a woman" and "a huntress." Then, he engages himself in a twisting, erroneous argumentation which may be summarized as follows: "If the scene [of JaT 102, top] is to be identified with inscription No. 1 [= JaT 102 a, B2], then it is suggested that ... we know her name to have been thbb."JaT 102 D cannot be the engraver of JaT 102a (B2) because the author's name is hylt (B1), and the engraver of B2 is thbb. Finally, thbb cannot possibly be a woman since it would be the subject of the following masculine verb htt.

c - In his reading of four short texts which do not present any special difficulty, Jobling shows both his incompetence and carelessness in the following errors:

in JaT 101: the fourth letter recorded in his facsimile is eliminated from both his transliteration and translation;

in JaT 102a: he misreads "g" (= t) instead of y;
he misreads "w," a letter which could be either w or ʿ but is (w) because of the context;
"s" instead of k; the letter involved is the upside-down reverse of the letter of JoDD 6 which Jobling read as a boubtful "ẖ" (cf. above, p. 171);

in JaT 102b: although recorded in his facsimile, the text is eliminated from both his transliteration and translation;

in JaT 104b: he misreads "z" instead of d;
his facsimile eliminates the third letter of the initial name;
he misreads "b" instead of r; and
he invents the inexistent "n" of bis second "bn."

d - As in Saf, the number of personal names is very high in the four texts released by Jobling, viz. there are ten of them, one being repeated twice, against a single verb repeated four times. Such a situation proves once more how obvious the importance of onomastics really is. Jobling never gives any etymological note, except for thbb referred to HaAIC where the name is obviously related to the root hbb. It is, therefore, impossible for the reader to know Jobling's onomastic interpretation of any other name. One characteristic example is the name kʾt of JoPRS 3/1 and 2.

e - Jobling's remark on wʿl of JaT 102 a/3 in relation with Ar and Hebrew is a useless rehash of a well-known information. However, such a repetition gives the reader the impression that its author does not know that the word wʿl is not a novelty at all; it has already been known in NA and SA for many years.

f - Disregarding the actual location of the texts, Jobling makes his first two texts to begin with the conjunction w and.

g - Except for three references to publications in footnotes 6 (p. 215) and 7 (p. 217) and a lonely mention of HaAIC (p. 215A), Jobling's commentary is strictly limited to the statement of his two theses.

(1) Jobling's first thesis deals with the value of the article.

((a)) Jobling's first statement is a naively ridiculous remark which reads as follow (p. 215A-B):

" the definite article is *b*." "

" Cf. (i) W. Fischer (ed.), Grundriss der Arabischen Philologie, Band I, Wiesbaden 1982, p. 20; (ii) F. V. Winnett, *A Study of the Lihyanite and Thamudic Inscriptions*, Toronto, 1937, p. 16ff. "

((1)) Jobling himself

transliterates the article as h- in both JoPRS 2/2 (p. 215A) and 3/1 (p. 215B). What is the reason for transliterating it differently here as "h-"? This is not a printing error since it is repeated in p. 217A (cf. below). What could Jobling possibly have in mind when he wrote his statement? Why does he not explain himself?

((2)) What is so "interesting" about an old-timer, such as the article h-? Why does he not spell out what he finds so "interesting" about it?

((3)) The first reference of footnote 6 is W.W. Müller's contribution on Tham, which is nothing but a rehash of well-known facts. But, here again, Müller indulges in phantasy when he adds the following remark: "vielleicht mit Geminierung des folgenden Konsonanten wie im Hebräischen"(also on p. 20). Müller does not have the slightest proof of such a possibility. His longest contribution to NA in W. Fischer's book is devoted to Saf; JaMAR, XIV, pp. 144-170, has demonstrated that all that Müller does is to compile some publications of his liking, including their errors, at the expense of some authors of his disliking. That is Müller's concept of "Wissenschaft."

((4)) The section of WiSLTI, pp. "16ff." (= pp. 16-18), is introduced by the title "The Lihyanite Article," an information which Jobling fails to give his reader who will naturally think that those lines deal with Tham. But, who can say that Jobling read the title?

((b)) Jobling's second statement, which is wholly unintelligible, is as follows (p. 217A):

> "Both examples, the first (AM85/100/12) with the definite article *(h)* and the second (AM85/99/1) without the definite article provide good examples of the phonetic value of this particular grapheme."

The two referred-to examples are html of JaT 103 b/1, and tml of JaT 104 b/2.

((1)) Here again is the unintelligible "h."

((2)) How could the aforesaid examples concern "the phonetic value of this particular grapheme"? What could possibly be the meaning of Jobling's high-sounding statement? This is, in my opinion, a good example of Jobling juggling with words, not knowing what he is talking about.

((3)) The difference between those two examples has already been known for a long time (cf. already above, p. 183), viz. w^{c}l of JaT 102 a/3 and tml of Ja 104 b/2, although without the definite article, are determined by the material connection with ibex I and camel D, respectively; tml of JaT 103 b/1 also is already determined by the same material connection with JaT 103A, and this determination is further emphasized by the article which, in such a case, has a determinative value.

(2) Jobling's second thesis concerns the consonantic value of ḏ.

((a)) Jobling's foremost goal in his paper is to proclaim his endorsement of one of E.A. Knauf's concoctions and to offer what he considers to be an indisputable proof. That such is the case is evidenced by the fact that, as already pointed out above, two of the four photos come from an earlier season, that of 1983. Had the sole purpose of his report been to illustrate by some samples the petroglyths and texts of Wādī Ḫafīr, he doubtlessly had bagged during his 1985 campaign a documentation extensive enough to find adequate examples. Indeed, he states that, during his sixth campaign,

> "Thousands of inscripuons and outstanding examples of pre-islamic rock art have been located "(p. 212A).

Therefore, to go back to a previous collection of photos remains unjustified. In his simplistic and embryonic knowledge and approach, he wanted what he considered to be a self-evident example, that of gml. Thus, having found his "gml" in JoPRS 4, he looked for "hgml." But, apparently he did not find it; so, he looked for it in a previous collection of photos. His choice of photo of pl. 43 (with JoPRS 1) remains nonetheless unexplained because his photos of pl. 45 (with JoPRS 4) and 46 (with JoPRS 2) combined with his photo of pl. 47 undoubtedly were quite enough by themselves to give a good idea of both petroglyths and petrographs and all the more so since his plate 43 contains three t's only while his pl. 44 contains five more which he himself does not even consider. Who in his right mind can justify such illogical an attitude?

((a)) Jobling does not refer the reader to his first reactions to Knauf's theory in JoFS, pp. 198-199, which provide the reader with yet another example of his typical inconsistent argumenting method. He first mentions Knauf's suggestion in footnote 8 added to his commentary on JoFS 15 in p. 198B. Then, he upgraded that reference by inserting it in his commentary on JoFS 18 in the following p. 199B. Finally, he forgets all about the theory in his commentary on the immediately following JoFS 19 in the same page 199B.

((b)) Jobling unequivocally formulates his thesis as follows (p. 215B):

> "Of philological importance has been the number of drawings which, combined with their inscriptions, provides clear evidence that the grapheme previously identified as ḏ *(thā)* has a phonetic value of ǧ (ǧim). This grapheme consists of two circles joined by a vertical bar."

Jobling crowns his genial argument with a triumphant outburst coined in the following pompous slogan, viz.

> "Two outstanding and quite clear examples of this new epigraphic evidence were found in the Wadi Hafir" (p.215B, immediately after the

preceding quotation). Yet, there is absolutely nothing in either JoFRS 1 and/or 2 which might even remotely suggest his concoction. Jobling applies his scheme to three names, viz. his "ḥgg," "bglt" and "ḥglt." However, in the latter case, his reading of the second letter is erroneous; and the reading of ṯ in the first two names can easily be explained. It should be strongly emphasized that Jobling does not give a single word of commentary on any of the names although he should NECESSARILY have PROVEN two things, viz. (1) that the names WERE UNEXPLAINABLE with the normal reading of the letter as ṯ, (2) why and how the well-known 𐡈 = ṯ in all NA and SA languages had been SO DRASTICALLY changed to 𐡈 = "g." It is, in my opinion, plain dishonesty to simply sweep under the rug and hide from the reader not only the onomastic parallels suggesting the involved roots in NA and the corresponding roots in Ar but also the complete lack of palaeographical justification of his novelty. Here follows the study of the three names involved.

ḫtt of JaT 101 is read by Jobling as "ḥgg." The name ḫtt is already found, e.g., in HaLi 214, where E. Littmann refers to Ar "ḫathīth or ḫathūth 'quick'" (cf. HaLiSTI, p. 27); cf. also ḫt in JaT 10 a/1, the 4th verbal form ʾḫt of Dghty 56a, and the Saf ḫttt of CIS 2021; for the root ḫtt, cf. DAFA, III, pp. 2082B-2086A.

ḫtlt of JaT 101 is read by Jobling as "bglt." Cf. Lih ḫtl of JaL 18 b/1 and commentary in JaMAR, VII, p. 25, which refers to Ar "ḫutlat 'celebrity, fame'" (cf. PrLAL, I, p. 84B; and DAFA, I, p. 375B).

ḫylt of Ja 102 is read by Jobling as "ḥglt." The small circle contiguous to, and on the left of, the lower end of the shaft of the second letter is a feature of the stone and does not belong to the letter. Had it been located in the vertical axis of the shaft, as is the upper circle, it would be taken into consideration. Cf. the names ḫylh of Lih JsaL 79/1, ḫyl of Tham, e.g., JsaT 687; and in Saf, both the verb ḫyl of CIS 3750, and the name ḥḫyl of CIS 1582. Finally, even were the name to be read "ḫtlt" - but it is not -, such name would still be etymologically sound; cf. Ar ḫatila "ventrosus fuit" (cf. PrLAL, I, p. 343B; for the root ḫtl, cf. also DAFA, III, pp. 2088B-2090A), and the names ḫtl in both Tham, e.g., Hu 205/2, and Lih JaL 133/1, and mḫtl in Saf WH 1081.

((c)) Jobling repeats his theory in the second half of his commentary on JoFRS 4, viz. at the very end of the epigraphic section of his paper as if it were his last testament. There cannot be any doubt that, in his opinion, his statement is the bludgeon stroke against the opposite interpretation which is irretrievably lost and, therefore, that it clenches his position once and for all. His statement reads as follows (p. 217A):

> "In these two instances the apposite situation of the inscripuon to the drawing (in this case of a camel) and the established equation of the respecuve second and third graphemes with the phonemes *mim* and *lām* make it clear that the grapheme (or sign) filling the first consonant slot is *ǧīm*.'"
>
> "' See E. A. Knauf, Südarabisch. *ADAJ*, XXVII, 1983, pp. 587-596."

((1)) Knauf's theory is studied and rejected at length in JaMAR, XIV, pp. 21-32. ((2)) It is hard to believe that an author could be so childish as to present such grossly simplistic and superficial an argumentation. ((3)) Nobody will ever deny or even question the obvious relation between ḫtal of Ja T 103b and camel A;
ṯal of JaT 104b and camel D;
ḥwʿl of JaT 102a and ibex J.

But, to infer from the first two examples that ṯal must be read "gal" amounts to an insane gibberish. Indeed, Jobling does not at all heed the fact - provided he is aware of it - that one reality may be rendered by different words, each of which belonging to a specific root. An outstanding, yet well-known example, which is so easily understood, is precisely ... the numerous Ar words for "camel," e.g., ʾibil/ ʾibil, baʿīr, ǧamal, ḫuwār, saqb, qaʿūd, qalūṣ, nāqat, etc. Tham presents the same situation but to a much lesser extent than Ar because these two languages are very unevenly developped. So far, three different Tham words are attested as having the meaning of "camel," viz. gml, e.g., of Negev 11052/1;
ʾbl of Hu 412b (cf. JaMAR, V, p. 77); and
ṯal of JaT 103 b/1 and 104 b/2.

The Ar root ṯamala means "to remain, stay, reside, dwell, tarry" (cf. LaEL, p. 353B) and ṯamal "remanence, stay, residence" (cf. l.c., p. 354A); for the Ar root ṯml, cf. also DAFA, II, p. 1230A-1232A. The word ṯal may, thus, refer to a domestic camel, one that stays home and does not run in the wild, viz. the most valuable domesticated animal the bedouin will ever have. The idea of stability also is predominant in the derivatives of Ar ṯamala; viz.

ṯumlat "grain ... in the receptacle, bag;"
"water remaining in a watering-trough, or a vessel of any kind;"
"somewhat [remaining] of intelligence, and prudence;"

ṯmâl "an aider, a succourer;"
tamîlat "remains of food, or of herbage, or fodder, or fresh pasture and of fodder;" and
"the part of the belly of a man in which are the food and the drink" (cf. LaAEL, p. 354A-C).

If the basic meaning of the root tmala is actually applied to both humans and inanimate things, it certainly may also be applied to animals. Only a blind pan-Ar would exclude such a possibility from Tham on the sole basis of its absence from classical Ar.

Another example common to Tham and Ar is the word for "ibex:"
w'l in Tham, e.g., Ḫâ'il 188+187, and Ar wa'l; and
'yl, a generic word, in Tham Hu 343, and Ar 'îyal/'uyyal/'ayyul.

((4)) Etymological study of other nominal and verbal forms of JaT 103 which Jobling did not deem it worthy to consider or comment upon or even register; tff is added to the list.

tff of Ja 103 b/2, which Jobling reads "gff," is the imperative of the 2nd form of tfy; cf. Ar tafâ (i), 2nd form "to put the cooking-pot upon 'atâfî" (cf. LaAEL, p. 351B, and DAFA, II, p. 1188B) or stones ad hoc.

tlmt of JaT 103d which Jobling should read "glmt." Cf. the name tlm, e.g., HaLi 415, and in Saf, e.g., CIAS 220. For the Ar root talama/talima, cf. DAFA, II, pp. 1222B-1224A.

mt of JaT 103e which Jobling should read "mg." Cf. Saf mtw of CIS 3168; it is difficult to figure out the reason why this reading is placed by HaAIC in his list of "doubtful readings"(p. 939A). Cf. Ar matta "to sweat by reason of fatness" (cf. LaAEL, p. 2688C). HaAIC (p.643) lists "wmt uncertain" in HaLi 160 from a "root unknown." The text reads as follows: lwm gn By Wasîm. He was protected [here].- wsm, Hu 684. - gn, cf. Ar ǧana (o) "custodivit ab aliquo" (cf. FrLAL, II, p. 534B); here, in the passive voice.

lt of JaT 103f which Jobling should read "lg." This verb is also attested in HaLi 455.

bltt of JaT 103 h/2 which Jobling should read "blgt." Cf. the names 'blt of JsaT 221; and Saf blty of Wi 893; cf. also Ar balîṭ "tender, soft" (cf. DAFA, II, pp. 786B-787A).

ytl of JaT 103 h/3, which Jobling should read "ygl," is the imperfect of ġl; cf. Ar ġâla (o) "to be, become, disordered in one's intellect," and ġawl "madness" (cf. LaAEL, p. 365B); for the root ġwl, cf. DAFA, II, p. 1268A-B.

Needless to point out that Jobling's reading of all the texts he discarded remains unknown.

h - At the end of his translations of JoPRS 1, 2 and 3, Joblings adds "(of it)" and "it," respectively. Such an initiative is unjustified. (1) Here again, Jobling is inconsistent with himself; therefore, his use of no parentheses in the first case and his omission in the second are illogical. Another consequence of this initial remark is that each addition should have been put between brackets, as does his treatment of JoPRS 4. Cf. already above, p. 168.

(2) In each case, Jobling carefully avoids explaining what "it" or "(it)" refers to. The paragraph containing the first translation deals with an "inscription" (p. 214B); thus, "(of it)" necessarily refers to the text alone. In the second translation, "(of it)" necessarily refers to the ibex which immediately precedes kll. In the third translation, nothing suggests that "it" refers to anything but the text. In the first text, kll certainly refers to both the inscription and the petroglyths; so does htt of the third text because C1+C2 and the petroglyth A form a single entity; so also does htt of the second text because ibex J is unintelligible without bowman I and the nine dogs surrounding the hunted animal.

4 - The inscriptions.

JaT 101 = JoPRS 1 (pp. 214B-215A, facsimile on p. 214, and photo in pl. 44, p. 342). For the petroglyths, cf. above, p. 179; for my tracing, cf. also above, p. 175. - The inscription is composed of five sections, viz. B1 in the upper center of the stone, a line aslant to the right with the last two letters making a senestrograde line perpendicular to the last letter of the aslant line; B2 with six letters forming a line aslant to the left; B3 is a senestrograde, horizontal line of 4 letters below and to the left of the bottom of B2; B4 is an almost horizontal, senestrograde line added to the left of the 4th-5th letters of B2; finally, the single letter of B5 is found above the 4th letter of B4. Jobling distorts the normal sequence of the text by taking B2 as the beginning of the inscription and by considering the beginning B1 as 1.2; he also betrays the order of B2 - B5 by adding B4+B5 after the 4th letter of B2. The preceding distortion of the the engraved text is neither mentioned nor justified in the commentary. An identi-

Jobling's reading of the text is as follows:

" *Transliteration:*	" *Translation:*
1 *W hgg. bn. bglt. ḫff. kll*	1. And hgg son of bglt drew all (of it)
2. *l. 'm. bn. smdt* " (p. 214B).	2. By 'm son of smdt " (p. 215A).

Jobling fails to note that he had already published the text in JoAMA, II (p.47, photo on p. 46, but no facsimile) where his reading is as follows:

" 1. whtt bn bglt ḫff kll 2. l 'm bn smdt

and ḫtt son of bglt drew all (of it) By 'm son of smdt"

It is thus after the writing of JoAMA, II (published in the "Winter 1984; cf. the front page of TRASUS) that he became infatuated with Knauf's 1983 paper. His description of the hunting scene speaks of "two hunting dogs with jaws exaggerated for dramatic effect" (p. 46). Anyone who has ever seen a hunting dog attacking, or being attacked by, a wild animal (and this is far from being an extraordinary event) does not see any exaggeration in the designs of the petroglyths. Finally, the siglum of the photo in the first edition is "AM83/37B/22" which becomes "AM-83/37/22" (p. 214, reproduced above, p. 175, and pl. 43). Jobling does not even mention the omission of "B" whose meaning remains a mystery. Note also that JoAMA, II, p. 45, publishes again a photo already printed in JoMoFSA, pl. 68B (cf. JaMAR, XIV, p. 124); and the same JoMoFSA, pl. 67 publishes the same map as in JoPRS, p. 194. - The text may be read as follows:

↘	l'mn bn smdt	1 (B1)	By 'Amîn, son of Samdat.
↙	whtt ḫṭ	2 (B2)	And Ḥaṭûṭ has
←	ṭ kll	3 (B3)	cut all.
↙	bn btl	4 (B4)	, son of Bat-
	t	5 (B5)	lat,

'mn: Jobling eliminates the third letter from the text of his publications while registering it in his facsimile (cf. already, p. 182); cf. also, e.g., Hu 415. - smdt, cf. smd of Eut 613. - ḫṭṭ, cf. WiAM 2b, 30 and JsaT 692. - B2 shows six letters axed in a straight aslant line; there is no way of introducing B4+B5 into B2 as if they would follow the fourth letter of B2, as Jobling does. Another example of the two elements of the author's filiation being separated from each other by the verb ḫṭṭ is JoDD 6, cf. above, p. 170.

aT 102 (cf. JoPRS, p. 215, facsimile in p. 216, and photo in pl. 46, p. 344).

a = JoPRS 2. - The text is composed of three sections. B1, the first section of the text, an almost vertical line to be read from top to bottom, and to the right of, both dog A and ibex E. To the left and slightly below the level of the bottom of the preceding section starts the second section, B2, a vertical line aslant to the right with the lower extremity curved to the right, to be read from top to bottom, to the left of ibex E and to the right of dog F. The third section of the text, half a circle convex to the left, begins below the hindlegs of dog K. Here again, as already stated above, Jobling's understanding of the text is so aberrant that he considers the first, upper section, B1, as the last of the text and, consequently, his text begins with the conjunction of B2, *w* and, - Jobling's reading of the text is as follows:

" *Transliteration:*	*Translation:*
1. *w. thbb. ḫff*	1. And thbb drew (it)
2. *w. simt. ḫff h. wʿl. kll*	2. and simt drew the ibex all (of it)
3. *l. bglt*	3. by bglt " (p. 215A).

In Jobling's transliteration of 1.2 in Ar letters, correct " وسم" to وعل. As stated above, p. 182 , it is because of his ridiculous understanding of man 3 that thbb becomes a female personal name and the subject of the masculine verb ḫff. - The text may be read as follows:

↘	lḥylt	1 (B1)	By Ḥaylat,
↘	wtḥbb ḫṭṭ	2 (B2)	and Taḥabb has cut [this],
↘	(w)klnt ḥ]ṭṭ hwʿl kll	3 (B3)	(and) Kalmat has cut this ibex entirely.

ḥylt, cf. ḥyl, e.g., in Ḥâ'il 190c, and Ar ḥaylat "perspicacitas, techna, astutia" (cf. FrLAL, I, p. 452A). - thbb: Jobling refers to HaAIC, p. 129, without noting that the latter refers to Saf only. The verbal form ḥbb is well-known in Thaj; cf., e.g., Ḥâ'il 57. - ḫṭṭ (B2) and ḫṭṭ hwʿl kll (B3): if the object of the first ḫṭṭ appears to be the upper hunting scene, kll of B3 makes difficulty because it is superfluous to add it after such small a petroglyth as that of a wʿl. The mention of kll may be explained when wʿl is given two different meanings, viz. the apparent meaning of "ibex" with reference to ibex J, and the understood meaning of a part designating the whole, viz. ibex J itself and the whole lower hunting scene centered around the same animal (cf. above, p. 189). - The inside of the conjunction (w) is completely destroyed, contrary to Jobling's facsimile which distorts the letter so much that it shows a letter perfectly clear, including the medial bar. - klnt: Jobling erroneously reads the name as "slnt;" the upper end of the letter is curved, not straight. Cf. Hu 747.

b : although drawn in Jobling's facsimile, this word is not even mentioned in his commentary.

lm Lawm.

Here, it is the name of dog K. The dog's name is given because of its outstanding performance: it is indeed the only dog among nine to have actually reached ibex J, and it grabs one of the latter's hindlegs in his muzzle. Cf. also the personal name, e.g., in NegevT 2. Cf. below, another dog's name in JaT 106z.

JaT 103 (cf. JoPRS, p. 215A, facsimile in p. 217, and photo 44 in p. 342).

a = B = a: the first half of the text is a vertical line to be read from top to bottom; and below it, the second half a slight concave, senestrograde line; the whole text is nicely cradled above the withers of camel A and between the hump and the neck of the same animal.

mhd 1 Mahad

lṯ ʾtl 2 has hidden ʾAtwal [here].

mhd, cf. Saf, e.g., CIS 4471. – lṯ, cf. Saf, e.g., Wi 262b. – ʾtl, Ṣâʾil 38.

b = C1+C2 = JoPRS 3. – C1 is an aslant line to the left of, and parallel to, the neck of camel A. The second half of the text, C2, is composed of three boustrophedonic sections, viz. three dextrograde letters below the belly of the camel, six letters written almost vertically, alongside and to the left of the hindlegs of the same camel, and the last three letters making an horizontal line slightly aslant to the left, between the penultimate letter of the preceding section and the feet of the forelegs of that camel. – Jobling reads the text as follows (p. 215B): "*Transliteration* *Translation:*

1. *l. k't. h. gml* 1. By k't the camel
2. *w. k't. bn. gff. kṭṭ* 2. and k't son of gff drew it"

The text may be read as follows:

lkʾt ḥtml 1 (C1) To Kayʾat [belongs] this camel,

wkʾt bn tff ḥṭṭ 2 (C2) and Kayʾat, son of Taff, has cut [this].

kʾt, Hu 62/2. The second kʾf gives his father's name to indicate that he is different from the owner of the camel. – ḥṭṭ: the complement of the verb is the whole unit composed of animal A and the accompanying text.

c = D: the first letter is senestrograde and is followed by a vertical line aslant to the right and to be read from top to bottom; it begins at the same level and to the left of the three initial letters of text a.

l 1 By

ḥwfl 2 Ḥawfal.

ḥwfl, a qwtl nominal derivation; cf. ḥflw of JsaT 248, which is read "ḥ-f-l w..." by vdBrIT (p. 376) because of his claim that the text is incomplete. But Jaussen-Savignac's facsimile does not show any hatching after the letter w.

d = E: a line aslant to the right, to be read from top to bottom, parallel to, and to the right of, the hindquarters of camel A.

lṯlmt By Ṯalmat.

e = F: an almost vertical line to be read from top to bottom, between the end of text b/1 and the end of text c – beginning of text f.

lmt By Matt.

f = G: an aslant line written below and in the axis of text c and to the left of texts e and g, and to be read from top to bottom.

lṯ dlt Has remained [here] Dawlat.

dlt, cf. Saf, e.g., dln, e.g., of JaS 177.

g = H: a senestrograde, concave horizontal line aslant to the left, and below texts b/1 and e.

ld hrs Has taken refuge [here] Hâris. – ld, JsaT 749. – hrs, cf. Saf Tap 2, and Tham hrsʾl of JsaT 242, and hyrs of Eut 590b.

h = J: the text is composed of three sections, viz. J1, a short vertical line aslant to the right, parallel to, and to the left of, the forelegs of camel A; J2, a dextrograde, undulating horizontal line below camel A; and J3, a senestrograde line to the left of J2 and below texts f and g.

lṯ 1 By Ṯa-

f bt blṯt ʿls ḥml 2 ff, daughter of Balṯat. Has eaten [here] Ḥâmil.

yṯl ṣrw 3 Is mad Ṣâtiw.

ṯf is masculine in Saf, e.g., CIS 1566. – bt, e.g., JaT 37b. – ʿls, Hu 405d. – ḥml, HaLi 521. – ṣrw, cf. in Saf ṣrw, e.g., of CIS 4685.

i = K: a dextrograde line below the preceding text and just above the upper edge of a long crack.

lm rʾʿ dhbn msk\˙

Has halted [here] Râʾiʿ, he of Hawbân. He was caught [here]. Wasm.

lm, e.g., Hu 137. – rʾʿ, cf. Saf rʾʿh of CIS 5304. – hbn, Hu 4. – msk, cf. Ar masaka "tenuit apprehensum, prehendit" (cf. FrLAL, IV, p. 179A); here, in the passive voice. – Wasm \˙ , cf. ·— of Eut 63 and 94 (cf. JaMAR, V, p. 125B), and ·— of Eut 184/1, and |·· of Eut 440 (cf. JaMAR, l.c., pl. 8B and 23B, respectively).

The panel shown in the photo of pl. 44 is larger than that of my tracing which represents only the space delimited by the upper edge of the stone and three cracks. Outside those borders, the photo shows partial sections of other complexes, viz. on the left, the right half of an ibex in the upper section;

and a cameleer looking leftward in the lower section; below, a long inscription of which are visible only the upper extremities of many letters as well as the first two, lḥ; on the right, the left 3/4 of a camel looking rightward.

JaT 104 (cf. JoPRS, pp. 215A, 217A, facsimile in p. 218, and photo in pl. 45, p. 343).

a = A: in the upper section, a dextrograde, straight line aslant to the left, and above the saddled, walking camel B which is looking rightward.

l³nʿm bn br By ³Anʿam, son of Barr.

³nʿm, e.g., HaLi 481; the second personal name of this text is m.k³l; but, the editor (cf. HaLiSTI, p. 44) reads it as "m(l)k³l," which becomes "Malak-³ēl" in his translation; thus, with the erroneous assumption that his restored letter is certain. Many other restorations are equally possible, and s should certainly be a favored. The end of the text makes difficulty: the editor reads it as "b m³" and E. Littmann states that it "may mean 'at the water'." But, the initial letter is a large r, and the b of bn is angular. The end of the text may be read as rm³ [who] has dwelled [here]. Cf. Ar rama³a "substitit, commemoratus fuit" (cf. FrLAL, II, p. 190A). - br, e.g., JaT 33a.

b = C1+C2 = JoPRS 4. C1 is a vertical line slightly curving rightward; it starts at the level and to the left of the tail of camel B, and runs toward the hindquarters of camel D. C2 forms a senestrograde line with the second half strongly curving downward. Jobling reads the text as follows (p. 217A):

" *Transliteration:*	*Translation:*
l. zḥ. bn [ ] bn. ʿm. ḳṭṭ. gml	By zḥ son of [ ] son of ʿm he drew a camel."

The preceding case of incompetence and erroneous reasoning is manifest. The second letter is erroneously read "z." - Contrary to his novelty, there is no dot after the first "bn" in his transliteration. - Jobling's suggestion of some letters missing in the center of the text remains unjustified and is unjustifiable. Both the position of the camel in relation with the text and the contents of C2 indicate that the animal was drawn after bn of C1 and before C2. Jobling's suggestion entails that the worker deliberately engraved some letters atop the engraving of the animal's body where they are illegible. Such a suggestion is absurd because nobody would ever engrave letters that would be illegible. That the worker did not execute his engraving perfectly is noticeable in h. However, although incomplete, the reading of the letter is ascertained; thus h¹. The error of Jobling's suggestion comes from his misreading of the initial letter of

as "b" (the letter is r because it is well rounded, contrary to b which is angular) and his gratuitous restoration of "n," which should have been put between brackets, between that letter and "ʿm." Finally, he eliminates the fourth letter of C1. - The text may be read as follows:

ldḥn bn	1	By Ḏawḥān, son of
rʿm h¹ṭṭ tml	2	Rāʿim. He has cut [this] camel.

dḥn, cf. the verb dḥ in Hu 598+599/1. - rʿm, Negev 10682.

c = E: on the left of camel D, cameleer G and ibex H, a slightly concave vertical line.

lkʿm bn sʿd bn dk By Kāʿim, son of Saʿad, son of Ḏāk.

kʿm, cf. kʿ(m) of Eut 231/1; the name is attested in Saf ClAS 580. - sʿd, e.g., Cadès 5. - dk, cf. Ar dāk^in (active participle of dāka, o) "diffusing odour, or having a strong [or pungent] odour" (cf. LaAEL, p. 972B).

d = F: a vertical line to be read from top to bottom, to the right of the the center of the preceding text.

lqq ³yl By Qīq. He has returned [here].

qq, JsaT 482. - ³yl, cf. Ar ³āla (o) "to return" (cf. LaAEL, p. 125C).

E - JoAMA, III (cf. AfO, 33 [1986], pp. 233-247A) publishes two more panels of petroglyths and petrographs.

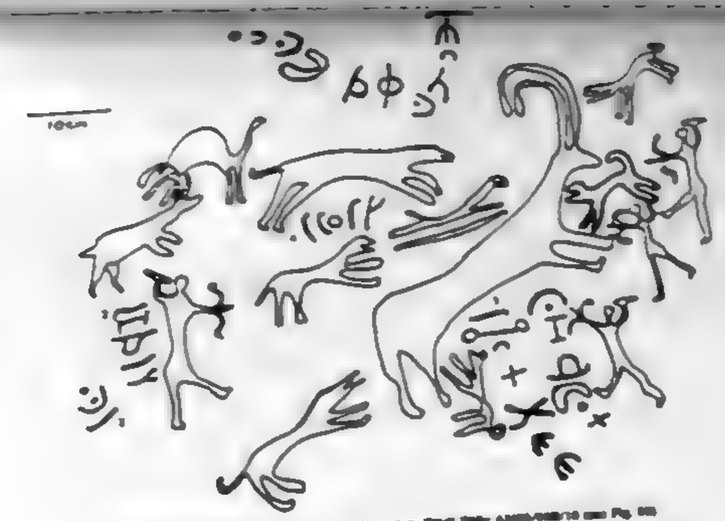
Preliminary remark. - Jobling introduces in his paper a brand new spelling, viz. "Wādī Ḫāfir" (passim, except in the titles of pp. 238 and 239 where "Hafir" reappears); thus, the lengthening of the wrong vowel; cf. already above, pp. 172-173.

1 - Jobling's work.

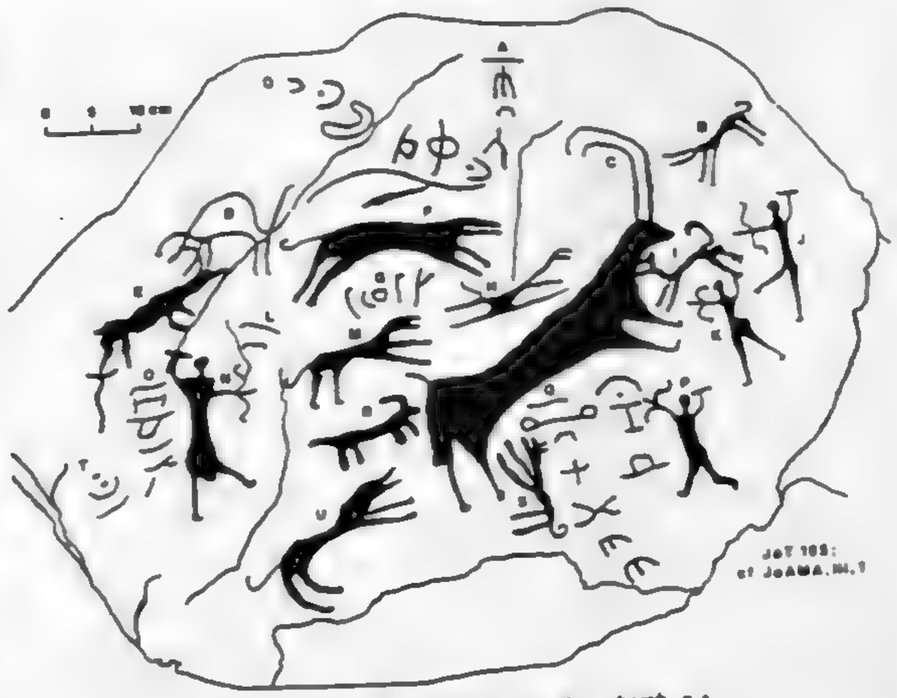
Jobling's remark to the effect that "these are only preliminary studies" (p. 243A) does not, and cannot, excuse or justify the dilettantish manner with which he handles the documentation; it is only a paltry pacifier aimed at lulling the reader so that he may close his eyes to an unacceptable practice.

Another remark on all the petroglyths found in that region reads as follows (pp. 243B + 245A): "Panels such as these indicate a polarity of relationship between the inscriptions and drawings and may on further analysis lead to a satisfactory elucidation of the ethnoecology of the area and world views upon which such drawings are based."

Those petroglyths may be a revelation for Jobling, but it has been known for a long time already that both Tham and Saf people depended on their camels and hunting for their daily life and survival. High-sounding expressions, such as "world views," in the present case are well in line of pedantic journalism. Besides, as in JaT 106, camel A has no relation with the texts and, therefore, it is ridiculous



(cf. JoAMA, XXX, 1, p. 242)



A = text a; L = text c; Q = text e;
C = text b; O = text d; T = text f.

(cf. JoAMA, XXX, 1, p. 242)

B = text a; F = text i; J = text d; N = text m; V = text p; AH = text t;
C = text b; G = text h; K = text e; Q = text n; X = text q; AI = text u;
D = text c; H = text g; L = text f; R = text o; AA = text s; AJ = text v;
E = text j; I = text k; M = text l; T = text r; AC = text r; AK = text w;
AL = text x; AQ = text y.

...e book in that petroglyph "a polarity of relationship between the inscriptions and drawings." If the camel-er represents his camel and does not write any text, it is simply because he likes his daily, faithful companion that means so much to his ... in the same manner that a farmer takes care of his horse first and secondly of himself.

No. 1 (cf. photo in p. 241, facsimile in p. 242, and readings in p. 243) = JaT 105. Jobling's readings are as follows:

"Transliteration: / Translation:
1 L HRS BN QDM BN ʾ / By HRS son of QDM son of ʾ
2 L ZDLH / By ZDLH
3 L BNN / By BNN
4 L ʾ'LH / By ʾ'LH
5 L GRT BN ZDMT KT / By GRT son of ZDMT he drew (it)"

As usual, there is no commentary on the readings; there is no remark on the hunting scene; and the facsimiles are, here again, mere superficial approximations. Since Jobling is most certainly not blind, there cannot be any justification for representing as simply delineated all the figures which are filled with stippling, except camel D. – The short text, L, iber B, and the wasm of text d are not found in his facsimile. – Finally, he misreads k as "s" in his no.1, t as "ʿ" in his no. 5, and keeps transliterating h as "ḥ" in his no. 5.

No. 2 (cf. photo in p. 244, facsimile in p. 245, and readings in p. 243) = JaT 106. Jobling's readings are as follows:

"Transliteration: / Translation:
1 L ʾLT BN WDD / By ʾLT son of WDD
2 L ʾMXN BN FT / By ʾMXN son of FT
3 L ʾSRS / By ʾSRS
4 L LD BN ʾSLM / By LD son of ʾSLM
5 L ʾBS / By ʾBS
6 L MR'LH / By MR'LH
7 L DR / By DR
8 ʾWDLH / By ʾWDLH
9 L MR / By MR
10 L MCL / By MCL
11 L SRQT / By SRQT
12 L BN BSR BN NGR / By the son of BSR son of NGR
13 L LB'T / By LB'T
14 L BTLT / By BTLT
15 L SLMT BN ʾMR'L / By SLMT son of ʾMR'L
16 L MRKT / By MRKT
17 L BT HDT / By BT HDT or by the daughter of HDT
18 L RHS BN SRY / By RHS son of SRY
19 L TKL / By TKL
20 L MR'LH / By MR'LH
21 L TWT / By TW'T
22 D YZNG (?) / This is YZNG (?)
23 FDH / Disgrace (or Reward (?)
24 L BT / By BT
25 L SKN / By SKN "

...re again, no commentary on the readings is volunteered; the facsimiles are of the same quality as in no. 1; with the unexplicable exception of iber A3, all other figures of his facsimile are delineated only. However, Jobling writes that (p. 243) "Quite distinctive is the packed camel dominating the upper centre right of the panel. This contrasts with the stick-figure drawings dominating the centre left of the panel and the hunting scene in the centre right quadrant."

This statement is a good example of Jobling's research. Here again, what are the elements of that "hunting scene"? Jobling, of course, does not say. Furthermore, it is precisely in his "centre left of the panel" that he places his only animal inked in, iber A3. Not only ALL figures are in stippling on the photo, but Jobling proves himself unable to distinguish in his facsimile between delineation and stippling in the hunted animal ʿ, which is also in stippling with a large rectangular blank in its body (cf. already above, pp. 181-182). Frankly, instead of doing a careful study and presenting the material as it is, Jobling seems to be rather fond of things pompous or high sounding whether or not they are related to the publication. The best example is his fig. 102 which occupies the whole p. 246 but has NO connection with the two published panels. The only apparent reason for that fig. 102 is simply to show off.

Jobling's aforesaid statement is immediately followed by a most puzzling suggestion which reads as follows (p.243):

"It is perhaps worth noting that in this hunting scene the object of the hunt may be the lioness drawn beside the name LB'T". "(1) Idem, p. 508."

The "idem" of footnote 21 should normally refer to the publication mentioned last. It is not so here: it refers to the penult book cited, viz. HaAE. What is Jobling's reason for selecting his no. 13 as being "beside" animal W rather than his nos. 18 or 14? The only apparent reason is that that Ar lab'at means "lioness," as his footnote refers to HaAE, p. "508," which gives the Ar word. But, then, his statement apparently suggests that, in his opinion, animal W is connected with his no. 13 which, in turn, would spell out the kind of animal W really is. Such a reasoning is at home with WiEaI's absurd interpretations denounced in Orientalia, 48 (1979), pp. 505-508.

Errors found in Jobling's readings of his no. 2:

1) his transliteration includes here again "T" instead of ṯ in his no. 16, and omits the initial letter of his no. 8, which reappears in his translation;
2) his reading confuses ṣ and ḍ in his no. 7, ḫ and ṣ in his no. 18, and ʾ and ʿ in his nos. 3 and 5;

3) [illegible] the following letters, viz. b of bn, and ṣ in his no. 12; the penult letter of his no. 16; four letters in his no. 22; and the first two letters of his no. 23;

4) his facsimile eliminates the third letter of his no. 19, and the wasm of both his nos. 13 and 15;

5) his mistreatment of his nos. 17 and 22 is the proof of his incompetence;

6) his translation eliminates text z and the last letter of his no. 22; invents two novelties, viz. a text beginning with either "the son of" in his no. 12, or "the daughter of" in his no. 17; and a double personal name, viz. a name composed of two independent sections, in his no. 17.

Jobling had already twice published a similar interpretation involving "bn son of" as the initial element of an inscription in JoPRF, pp. 205-206. It is a simplistic approach to think that "bn" necessarily means "son of."

JaT 128 = JoPRF 7 (p. 205B, and photo in pl. 46A) which is read by Jobling as follows: " *bn ḵmr* Son of *ḵmr* = My tracing is given here:

The text may be read as follows: bn ḥmr

Has halted [here] Ḥamir.

bn, e.g., JaT 9/3. - ḥmr, e.g., Saf LP 980. - The preceding formula is much more frequently used than the following which would call for the translation of the text as Bayyin has dwelled [here]. - bn, e.g., Hu 443, and ḥmr, Hu 266a.

JaT 129 = JoPRF 16 (p. 206, and photo in pl. 45B) which is read by Jobling as follows: " *bn ḥdḍ* Son of *ḥdḍ*" See here my tracing:

Jobling's commentary is worthless except for his initial remark according to which "the name ḥdḍ is unattested in North Arabian." Such a remark does not prove anything; what he should have said is that the root "ḥdḍ" does not exist. - The text may be read as follows: bnḥ dḍ Was a meatcutter Daḍḍ. - bnḥ, Hu 4.- dḍ, cf. Ar daḍḍa "diligentissime ministravit" (cf. FrLAL, II, p. 31B).

JaT 130 = JoPRF 1 (p. 205, and photo in pl. 40B) which is read by Jobling as follows: " *w šḥdd ḵṯṯ*

And *šḥdd* drew (it). =

The panel of the stone illustrates several features dealt with in the preceding pages; see next page for my tracing.

1) In the following quotation (p. 204A), the present panel is explicitely listed in the first group, but is excluded from the second where it belongs, [illegible] adverb "also."

"As in previous years, there has also occured a considerable number of scenes which seems to be combined with Thamudic inscriptions (Pl. XL, 2). Also identified and photographed in this season's survey were hunting and pastoral scenes"

2) The error of the singular "seems" instead of the plural is not new; cf. already above, p. 161; and below, in H 2 d (7).

3) The drawing of the dog which is running at top speed toward the neck of the ibex is similar to that of Jobling's wrong facisimile of JaT 105 H (cf. above, p. 196). In the publication of the latter, Jobling does not refer to this photo.

4) The blanks in the three drawings show how erroneous is Jobling's understanding of them (cf. above, p. 180); cf. also the man in pl. 42B.

5) The second letter is here also understood by Jobling as "š;" cf. already above, p. 165, and also below in H 2 c (5).

6) Here again, Jobling transliterates ḫ as "k."

The text JaT 130 may be read as follows:

wl ḥdd ḥṯṯ Has much lamented Ḥudâd [who] has cut [this].

wl, cf. Ar wayyal "dixit alicui multum wayl (vae!)" (cf. FrLAL, IV, p. 514A). - ḥdd, Naveh 1. - ḥtt, as in the edition.

2 - JaT 105 = JoAMA, III, 1 (cf. above, p. 196).

a - The panel.

The panel is composed of a hunting scene and several, mostly peripheral, additions. The hunted animal C is a large, rearing ibex looking rightward. It is attacked both frontally and rearwardly. Facing the beast are three archers, J, K and P. While archer K stands alone, his two companions, J and P, are each accompanied by one dog, B and S, respectively. Furthermore, archer P engraved his text Q between himself and his dog S. Behind the hunted animal C is one archer N, who wrote his own name O behind him, is accompanied by four dogs, F, H, M and U. Thus, archer N appears to be the leader who is receiving help from three of his servants or friends. Note that Jobling's facsimile represents dog H as in JaT 130 (cf. above, this page).

The additions to the aforesaid hunting scene are as follows.

A = text a in the upper center of the stone;

D = a camel in the upper left section, and looking rightward;

E = an ibex below and to the left of camel D, and also looking rightward;

G = text b below dog F;

I = a dog to the right of the throat of ibex C, and looking leftward; its patina is whiter than that of the surrounding figures; besides, the animal is in the trajectory of the arrows of archers J and K;

L = text c above and to the left of the bow of archer N; not registered in Jobling's facsimile;

R = an ibex to the left of ibex C, looking rightward and also missing in Jobling's facsimile; and

T = text f below and to the left of text d.

b - The texts.

JaT 105 a = A = JoAMA, III, 1, 1: a vertical line making an acute angle leftward, in the upper center of the stone.

lḥrk bn qdm bn rᶜ By Ḥarik, son of Qadam, son of Rawᶜ.

ḥrk, e.g. Hu 238b. - qdm, JsaT 454. - rᶜ, e.g., HuIR 74.

b = G = JoAMA, III, 1, 4 = a dextrograde line between F and M and left of H.

lbᶜlh By Baᶜaluh. - HaAIC (p. 112) refers to Saf WH 1571, but the name is bᶜdlh, as in the edition.

c = L: completely eliminated by Jobling; a vertical line slightly aslant to the left, below and to the left of F and G, and above and to the right of the bow of archer N.

lb Labb. - Cf. WiAM 24.

d = O = JoAMA, III, 1, 2: a vertical line to be read from top to bottom, and parallel to the back of archer N.

lddlh Wasm By Dawdlah. Wasm.

ddlh, also in JaT 126b; cf. dd of HaLi 323, and ddᵓl of JaT 113/1. HaAIC's reference of "dd" to Saf 595 must be erased; cf. JaSN, p. 83, note 131. - The short aslant stroke below the text is too neatly engraved to be eliminated, as in Jobling's facsimile. It is a wasm; cf. Ḥāᵓil 139 (cf. JaMAR, XIII, p. 244B).

e = Q = JoAMA, III, 1, 5: two parallel lines to be read from top to bottom, between archer P and his dog S, and almost perpendicular to the belly of hunted ibex C; line 1 is on the left of l. 2.

[illegible] By [illegible]. He has sent [this].

bn dd(mnt?) 2 son of Dawd(manat?).

rrt, e.g., rr in WiTa 16/1, and Ar tawrat "excitement" (cf. LaAR, p. 363A). - dd...: the editor reads "[illegible]" (cf. V.A. Clark, below). But, the last three letters are not distinguishable in the photo although the body of archer P is; therefore, they cannot be drawn. For the author's pedigree being divided into two sections by ḥtt, cf. above, JoDD 6 in p. 170.

f = T = JoAMA, III, 1, 3: a vertical line aslant to the right, below and to the left of O, and to be read from bottom to top.

lbnn By Banîn. - E.g., Hu 363/2.

V.A. Clark continues to apply his dilettantish method in his paper of [illegible], ICJ (1987), pp. 185-191 and pl. 43-46. ClSTI 5 is published as Than (p. 188A, facsimile in p. 188A, and photo in pl. 46A, fig. 3a). He reads I as "r" (as Jobling); thus "ᶜnzt;" the name is ᶜndt ᶜAndat; cf. Saf hᶜnd in Saf CIS 1980. Clark refers his "ᶜnzt" to CIS 2958, which ClAS had already mentioned in the commentary on ClAS 1194, and which ClAS had copied from HaAIC, p. 444. The author states in footnote 6 (p. 190) that "CNSLJ 958 should also be added to this list," viz. ClAS 1195-1196 mentioned in his commentary on ClAS 1194. But, that ClAS 958 is already given under "ᶜnzt" in his list of proper names (p. 459B). The reference to CIS 2958 is a wrong number for it is impossible to read "ᶜnzt" there. The name of CIS 2958, which CIS (p. 388) reads "ǧddt," is ǧǧt Ǧaǧǧat; cf. Ar ǧaǧǧa "dispersus, disgregatus fuit (populus)" (cf. FrLAL, II, p. 430B). The editor could also have referred his "ᶜnzt" to Lih ᶜnzh of JaL 118/1. Finally, Clark states that his Ph.D. bears the date of "1980" (p. 185), a figure which he copied from the 1983 UMI print (front cover and p. C) altough p. 1 (the front cover) of his thesis has the date of "1979," and the end of the "PREFACE" (p. iii) has that of "25th October, 1979." Cf. my lenghty review of ClAS in JaMAR, XIII, pp. 2-113, and p. 112 for my study of ClAS 1195.

3 - JaT 106 = JoAMA, III, 2 (cf. above, p. 197).

a - The panel.

The panel is composed of 46 items, viz. 26 texts and 20 petroglyths, including one human only, archer Z. - The dominating petroglyth is that of the large she-camel A with her young P in the sucking position. - There is only one hunting scene in the middle right third of the panel. The center of the scene is a heavy-bodied animal W (for the blank in the body, cf. already above, pp. 179-180) growling at ibex U and targeted by archer Z, who has his quiver attached to his

upper back. For the representation of the bow of archer Z, cf. four more exampl in JoFRS's pl. 47 (cf. already above, p.180). Ibex U is pursued by dog Y. Text = AC may be considered as the name of archer Z; and text z = T that of dog Y. Among the remaining petroglyths, the representations of ibexes is overwhelming, viz. 12 of them, 7 looking rightward and 5 leftward, viz., respectively, C, AB, AE, AF, AG, AM, AP and S, AD, AN, AO, AS. Only two dogs are represented, viz.AR and AT. - Finally, all inscriptions are vertical lines to be read from top to bottom, and most of them are aslant to the right.

b - The texts.

Concordance of the texts, the sigla of my drawing and JoAMA, III, 2,nos.

text a = B = no. 10	text g = H = no.1	text n = Q = no. 18	text u = AI = no.23
b = C = no.11	h = G = no.6	o = R = no. 4	v = AJ = no.5
c = D = no.12	i = F = no.7	p = V = no. 13	w = AK = no.16
d = J = no.20	j = E = no.8	q = X = no. 14	x = AL = no.25
e = K = no.21	k = I = no.19	r = AC = no.24	y = AQ = no.17
f = L = no.22	l = M = no.3	s = AA = no.2	z = T
	m = N = no.9	t = AH = no.15	

Concordance of the numbers of JoAMA, III, 2, and my sigla.

1 = g; 2 = s; 3 = l; 4 = o; 5 = v; 6 = h; 7 = i; 8 = j; 9 = m; 10 = a; 11 = b; 12 = c; 13 = p; 14 = q; 15 = t; 16 = w; 17 = y; 18 = n; 19= k; 20 = d; 21 = e; 22 = f; 23 = u; 24 = r; and 25 = x.

JaT 106 a = B=JoAMA, III, 2, 10: a line aslant to the left, and left of the top of the vertical axis of the stone.

lmṭl By Maṭṭâl. - E.g., Saf LP 901a.

b = C = JoAMA, III, 2, 11: aslant to the left, to the right of the preceding text, and above and perpendicular to the horns of ibex O.

lsrqt By Sarqat. - Cf., e.g., Saf Wi 965, and Ar sarqat "theft" (cf. LaAEL, p. 1352B). The name of Saf Wi 955 reads slqt Salqat.

c = D = JoAMA, III, 2, 12: two vertical lines to be read from right to left; the two lower extremities curve inwardly; between the preceding text and the large camel A, and also above and to the right of the horns of ibex O.

lbn bsr	1	Was struck was violence Bâsir,
bn nṣr	2	son of Naṣr.

lBn, here in the passive voice, e.g., as in Eut 610. - bsr: the name "bsr" read by HaAIC (p. 105) in WE 883, which is "b(k)r" in the edition, is bsl Bâsil. Cf. ...sert of JaaT 515. - bn: Jobling incorporates in his facsimile of b a dot inside the letter; and so he curves the left section of the letter inwardly. - Cf., e.g., HaLi 463. Also, apparently facsinated by the letter b, Jobling misses the right loop of the middle letter of the name, which is wide open in the photo.

d = J = JoAMA, III, 2, 20: the lower end of the line is slightly curved rightward; parallel to, and to the right of, the hindquarters of camel A and text k.

lmr'lh By Mara'lah. - E.g., HaLi 295. The present author also engraved his name as text h. It is impossible to determine which of the two texts was engraved first.

e = K = JoAMA, III, 2, 21: immediately to the right of, and parallel to, the preceding text.

ltw't By Ṯaw'at. - Cf. Tham tw' of Hu 27.

f = L = JoAMA, III, 2, 22: to the right of, and parallel to, the beginning of the preceding text. - Jobling confuses d and ḏ; gives the latter a demonstrative value; and invents the formula ḏ as introducing a Thas name. The two preceding ideas are inspired by the expression ḏn + name which is sometimes found either isolated or at the beginning of a text. But, the word ḏn is not a demonstative pronoun in any of those inscriptions (cf. JaTS, pp. 4-32). Finally, his transliteration and translation eliminate the last letter ' which is recorded in his facsimile. Jobling's reading reflects his facsimile which distorts the original completely. The first three letters are contiguous to each other. The initial letter is an angular f; the central letter is a rounded r; and the third letter is y. The fourth letter is a well-curved b; the dot contiguous to the right side of the letter is as adventitious to the letter as the two dots located, respectively, at the center and on the upper extremity of the last letter. The text may be read as follows: fry bn t' Farî, son of Taw'.

fry, cf. Ar farâ (i) "to split, cut lengthwise" (cf. LaAEL, p. 2391A). - t', cf. Ar tâ'a (o), 4th form "to shoot someone with an arrow" (cf. LaAEL, p. 361A, and FrLAL, I, p. 233A); cf. Tham t'nt of HaLi 21 which, incidentally, is listed by E. A. Knauf as an example of his erroneous theory of t = $ṯ$ (cf. ADAJ, 27 [1983], p. 587A: 1.5) because, according to him, "/t/ erscheint ausgeschlossen." If it so "erscheint," which cannot prove anything, it apparently is because he ignores, or is unaware of, the rare, but attested, qtlnt nominal derivation; cf. wlnt of Sut 77 a/1. Cf. also Saf t't in CIAS 1162.

g = H = JoAMA, III, 2, 1: in the upper left corner of the panel.

l ᶜlt bn wdd By ᶜUllat, son of Waddâd.

ᶜlt, e.g., JsaT 134. - wdd, e.g., Su 126, but feminine, e.g., in Ḫâ'il 63.

h = g = JoAMA, III, 2, 6: to the right of the preceding text, and beginning at the top of the rock.

↓ lmr'lh By Mara'lah. - Cf. text d.

i = F = JoAMA, III, 2, 7: a vertical line to the right of the first half of the preceding text.

↓ lgr By Gâr. - Cf. above, JoDD 1 (p. 170).

j = E = JoAMA, III, 2, 8: a line aslant to the right, composed of small, thin letters, and to the right of the preceding text.

↘ lᶜwdlh By ᶜIwâḍlah. - It is the first name of HaLi 65, of which the second name is wd Wadd (cf. JaMAR, VII, p. 138, note 116).

k = I = JoAMA, III, 2, 19: between the hindlegs of camel A and text d.

↘ ltkl By Takal. - Cf. JaT 7 a/2.

l = M = JoAMA, III, 2, 3: between texts g and h.

↓ l'lrs By 'Ilrâs.

Jobling endorses the old, fancy opinion of reading a straight bar as "š," e.g., as in HaLiSTI, pl. 26. This, of course, is very practical because it provides an advantageous selection of two values to choose from and, thus makes it possible to find a well-known name or to discard a difficulty (cf. already above, p.165). In the process, however, Jobling gives a good example of his lack of consistancy; viz. the left extremity of the bar is no more, no less curved than the right extremity of the initial letter of both his nos. 16 and 19 ... and Jobling never doubted the value of l of the two preceding letters because, of course, each of them was the lâm auctoris. A straight bar is twice the letter l in JaT 102a (cf. above, p. 176), also in JaT 104c and, again, twice in JaT 104d (cf. above, p.178). As the reader can see, inconsistency and fanciful decipherment often denounced in the past and especially in F.V. Winnett's publications, are far from being an item of the past. Cf. Lih yrs of JaL 1/6.

n = N = JoAMA, III, 2, 9: below text j and above ibex S.

↘ lmnr By Manâr.

Jobling's facsimile did not register the third letter. - Cf. JsaT 713.

n = Q = JoAMA, III, 2,18: two lines making a right angle, between the hindlegs of camel A and the hindquarters of animal W.

↘ lrhs 1 By Râhis,

→ bn sby 2 son of Sabî.

rhs, cf. Saf, e.g., CIS 5010, and rhsn in CIAS 1050. - sby: although the two s's are identical in form, Jobling gives the second the value of ṣ which is entirely different, as seen in the first letter of rhs. Cf. Saf, e.g., CIS 1514, sbyn in CIS 2334, and sbyt in CIS 1361; cf. also Thsm sbyyt in Hu 13.

o = R = JoAMA, III, 2, 4: it begins between texts g and l and curves rightward to pass under ibex AD and to the right of both texts v and ibex AM.

↘ llḍ bn 'slm By Laḍḍ, son of 'Aslam.

lḍ, e.g., Hu 111. - 'slm, e.g., HaLi 36; Saf, e.g., Damas 5537; and Lih JaL 101 b/2. The author has written the same text in JaT 120 (cf. below, H).

p = V = JoAMA, III, 2, 13: between the forelegs of ibex U on the left and on the right the muzzle of animal W and text q; the wasm is an almost vertical line to the left of the third and fourth letters.

↘ Wasm llb't Wasm. By Lab'at.

For the wasm, which Jobling does not record, cf., e.g., But 42. - lb't, e.g., JsaT 78, which HaAIC lists not only under lb't (p. 508), but also as a possibility under "lw't" (p. 939A). Contrary to HaAIC (p. 508), this name cannot be related to Ar "lab'at lioness" because all persons bearing the name are men. Thus, cf. Ar laba'a "to feed people etc. with biestings" (cf. LaAEL, p. 2644A).

q = X = JoAMA, III, 2, 14: parallel to, and to the right of, the preceding text, and to the left of the forelegs of animal W.

↘ lbtlt By Batlat. - Cf. Saf, e.g., CIS 2657.

r = AC = JoAMA, III, 2, 24: immediately below the bow of archer Z, and the latter's name.

↓ lbṭ' By Baṭî'. - Cf. Ar baṭî' "slow, tardy" (cf. LaAEL, p. 216A).

s = AA = JoAMA, III, 2, 2: immediately below text g, above the tail of ibex AM, and to the left of the end of texts o, v and the horns of ibex AM.

↓ lᶜmrn bn fṭ By ᶜAmrân, son of Fuṭ.

ᶜmrn, e.g., HaLi 103. - fṭ, cf. Saf fṭṭ, e.g., in Ox 298.

t = AH = JoAMA, III, 2, 15: to the right of ibexes AG, AP and AS, and also of text x.

↓ lslmt bn ᶜmr'l Wasm By Salmat, son of ᶜAmar'il. Wasm.

slmt, e.g., JsaT 655. - HaLi 55+56 is composed of 3 sections separated from each other by an empty space immediately after each bn of the text. HaLiSTI (p. 14, and pl. 4) takes the first section as his no. 55, the second as his no. 56, and forgets all about the third. The text reads as follows:

[illegible]

By ʾAmarʾil, son of ʿAmarʾil, son of Matt.

[illegible] also E. Littmann's fancy remark which immediately follows no. 56, "[The fo[illegible] [illegible]mar-ēl may be a variant of ʾAmar-ēl]." - The wasm, which is eliminated by Jobling, is a straight line whose extremities curve in opposite directions.

u = AI = JoAMA, III, 2, 23: below and the to right of the legs of archer Z.

lmḥ By Mayḥ.

Cf., e.g., Eut 404b. Jobling considers as a part of the initial letter two features of the stone; therefore, he reads the initial letter l as "f." He also traces as two concentric circles the letter m: the right lower extremity shows a curved line closing the gap. It is not perfectly clear whether or not the left extremity was also closed; it could be an angular broken line, as in text t.

v = AJ = JoAMA, III, 2, 5: between the second half of texts o and s, and above the horns of ibex AM.

lʾrl By ʾAryil. - Cf. Liḥ, e.g., JaL 169p, and Saf ʾrll in ClAS 905. For Jobling's theory on his "s̄," cf. above, p. 206.

w = AK = JoAMA, III, 2, 16: to the right of the beginning of text t.

lmrht By Marhat. - Cf. Saf CIS 228. Here, as well as in texts c and u, Jobling incorporates a feature of the stone into the design of the fourth letter.

x = AL = JoAMA, III, 2, 25: below ibex AG, and above ibex AP.

lskn By Sâkin. - Cf. Saf, e.g., LP 476, and ʾskn in Tham Ḫâʾil 189a.

y = AQ = JoAMA, III, 2, 17: parallel to, and to the right of, the lower half of text t.

lrt hdt By Ratt. [There was] thunder!

rt: the initial letter is a large r; the lateral sides of b normally are perpendicular to the main central bar. Here, those sides are even more curved inwardly than in r of text f (cf. above, p. 204). Cf. Saf, e.g., Ox 297. - hdt, cf. Ar hâdtat "thunder" (cf. LaAEL, p. 2883A), and the verb hd to ruin, e.g., in JsaT 277b. Jobling's two interpretations show once more his incompetence, viz. the first with a personal name composed of two independent sections, or the second calling for the beginning of a text to be "the daughter of," as he had already done in his no. 12, "the son of."

z = T: recorded in Jobling's facsimile, but unaccounted for in his readings; below ibex O and immediately above dog Y.

lq Lawq. - Cf. the name of a man, e.g., in Ḫâʾil 139b. Since dog Y be-

[illegible]ngs to the hunting scene and is located below the name [illegible] [illegible]duced by the lâm auctoris, I consider the name as that of the dog itself, [illegible] above, another dog's name in JaT 102b in pp. 191-192.

F - JoAM in Liber annuus, 32 (1982), pp. 467-470 and pl. 111-113.

This paper partly duplicates and partly complements JoAES of ADAJ, 27 (1983), pp. 185-186 and pl. 32-38. Both publications present the 1982 ʿAqaba-Maʿan survey, viz. the third season; yet, they never refer to each other. For instance, both publication have in common the following illustrations, viz.

"ʿAqaba-Maʾan [sic!] Survey 1982. Map of antiquities" (cf. pl. 111 and p.186, respectively);

"Tell el Kharaza dam and water chanels" (cf. pl.112A, where it is specified that the whole work is "Nabataean," and pl. 36A with the spelling "Tell el-Kharaza," respectively);

the photo of "Sadr Arqa" text (cf. pl. 113A and pl. 35A, respectively);

the photo of the "Wadi el-Leyyah" text (cf. pl. 113B and pl. 38, respectively); and

the photo of "Hedeib el-Fala" (cf. pl. 113C and pl. 36B with the spelling "Hedeib el Fala," respectively).

Jobling publishes a few Tham inscriptions. Surprisingly enough, however, it is impossible to determine their exact number (cf. below).

As far the epigraphic section of JoAM is concerned, Jobling introduces it in a most revealing statement, which reads as follows (p. 467):

> "The epigraphic and philological guidance of Mr. Michael Macdonald and consultation with Professor F. V. Winnett concerning the Thamudic inscriptions of the survey have greatly facilitated this aspect of the project and enabled a preliminary publication of some of this new material."

The preceding statement is supposed to be the same as as JoAES's parallel affirmation since the two of them describe the very same situation. JoAES's note reads as follows (p. 185B):

> "The continuing interest of Mr. Michael Macdonald in the recording and study of the Thamudic Inscriptions is also greatly appreciated."

It is manifestly impossible to equate the two preceding statements. The so-called "consultation with Professor F.V. Winnett" disappeared from the horizon as if it never existed, and Macdonald's "epigraphic and philological guidance" is reduced to his "mere continuing interest." Apparently, Jobling does not understand the

aning and value of the words he uses. In spite of JoAES's text, JoAM unequivo-lly states that the "preliminary publication of some of this new material" is e result of the combined studies of Jobling, Macdonald and Winnett. Such a dis-osure is of great importance because all the following criticisms against Job-ng's readings and interpretations automatically concern both Macdonald and Win-tt inasmuch as they actually contributed to the elicitation of Jobling's pub-shed text. Although no detail is released on Winnett's "consultation" or Mac-nald's "guidance," it seems logical to accept as a fact that they both gave their acet to the specific points emphasized or stressed by Jobling's readings and ar-mentations for the simple reason that it is precisely upon the authority of both innett's "consultation" and Macdonald's "guidance" that Jobling ultimately rests is case to the reader. Cf. below, the special case of JaT 109.

aT 107 = JoAM 1 (p. 469 and pl. 112C).

Jobling presents the text as being " Typical of such new inscriptions close to Wadi Ram " and which was found " just to the north of Wadi Ram ;" and e reads the text as follows: "*l 'obrq bn ḥddn bn tmlh*."

JaT 107

The group to which the text belongs is not referred to in oAES; and the number of the photo remains unknown. Jobling does not ention the cartouche encircling the text; nor does he point out that he text is boustrophedon; he erroneously reads the first letter of the author's ame as "ʿ" instead of ʾ and inserts the vowel "a" in his transliteration; he in-ludes an adventitious bar to the third letter and eliminates the waṣm at the end f the inscription. – The text may be read as follows:

l ʾhrq bn ḥddn	1	By ʾAhraq, son of Ḥudādān,
bn tmlh waṣm	2	son of Tamlah. Waṣm.

mlh, e.g., HaLi 415. – Waṣm, e.g., Eut 260/2.

Jobling introduces the two following texts as " Two quite novel contributions to Thamudic epigraphy and philology " because of the third letter of JaT 108 and the penult of JaT 109. Jobling's evaluation of these two letters is nothing but another proof of his incompetence because those two letters are not new at all and their value is not in doubt. But, the sigla of the two photos are "novel" because they are quite different from the normal "AMX/X/X" siglum, viz. "S.A. 06B.31" and "W.L. 14E/4." The letters are the initials of the names where the stones were found, viz. "Sadr Arqa" (cf. the same and صدر أرقة in JoPRF, p. 199) and "Wadi el-Leyyah." This name is found in four different spellings in Jobling's papers, viz. "Wadi el-Leyyah" (cf. JoAM, p. 469 and pl. 113B, and JoAES, pl. 37A and 38; already above, p. 209) because "Wadi el Leyyah" (cf. JoAES, p. 192B, where "Wadi el-Leyyah" is also found); "W. eh Leyyah" (cf. the sketchy map in JoAM, pl. 111 = JoAES, p. 186); and "Wadi al Layyah" (cf. JoPRF, p. 200: وادي الايه).

The approximation of Jobling's sketchy maps which are accompanied by a scale may be illustrated by the distance and the location of "Sadr Arqa" and "el Quweira:"

in JoAM, p. 111 = JoAES, p. 186 = AfO, 29-30 (1983/1984), p. 265: about 16 klm northeast;
in JoPRF, p. 198: about 12.5 klm east-northeast;
in JoFSA, p. 85 = JoFS, p. 193: about 15 klm east-northeast;
in Syria, 55 (1983), p. 319: about 19.5 klm east-northeast.

JaT 108 = JoAM 2 (p. 469 and photo in pl. 113A) = JoAES 2 (p. 189A and photo in pl. 35A). JoAM's reading of the text is explained as follows:

" The usual form of this grapheme in this area is that of a circle divided per pale. However in this inscription the circle is divided per saltire. This results in the quartering of the circle and the formation of a grapheme which is formally the same as Phoenician-Hebrew and Aramaic ṭet, but does not have the same value. It would appear that this is also the first occurrence of the name *kwm*."

The preceding text is repeated in JoAES as follows:

" *l.kwm* لكوم

Though a short inscription it would appear to be important in that it provides evidence of a variation in the *wāw* grapheme. The usual form of this grapheme in this area is that of a circle divided per pale.' However in this inscription the circle is divided per saltire and is formally the same as Phoenician-Hebrew and Aramaic *ṭet*, but does not have the same sound.'" (p. 189)

and " *l kwm* لكوم " (cf. caption in pl.35A).

First of all, the two different transliterations of the name should be noted, viz. "kwm" and "kwn" (twice), and both are erroneous!!! Furthermore, Jobling does not note whether the stone bearing the text is loose, as it appears in the photo. The photo is printed upside down because the dextrograde inscriptions are rare. Where is the proof that the third letter is one of the "two quite novel contributions"? That letter is well represented in Huber's collection; cf. also, e.g., JaT 33c, 34a and d. What is "novel" about it? What Jobling is doing is to repeat,

as if it were his discovery, an old tune of F.V. Winnett who, in WiSLTI (1937), p. 34, states without any proof that

" wāw—Wāw appears as a square or circle with a single line through it, or with two intersecting lines."

But, although the latter's pl. X mentions three such letters under w, one in his "A" and two in his "B," there is not a single example of them in pl. III (his "A") and IV (his "B"). Jobling's starting point is as wrong as Winnett's was more than 50 years ago, and he also follows Winnett's argumenting method: he does not give the slightest palaeographical reason which might justify his choice; he simply states it. There is no palaeographical possibility for ever connecting the present form with w which is n e v e r found with a cross inside any of the geometrical forms used as outside frame.

The senestrograde JaT 108 may be read as follows: ← lḥḍm By Ḥāḍim. – Cf. Ar ḥaḍama "to cut, cut off, cut to pieces" (cf. LaEL, p. 758A).

JaT 109 = JoAM 3 (p. 469 and photo in pl.113B) = JoAES 5 (pp. 192B–193A and pl. 38). – JoAM's reading of the text is as follows:

" it more importantly provides an example of the controversial Thamudic grapheme formed by two concentric circles. In the past this grapheme has been given the value of either Wāw (و) or ǧīm (ج). However the concurrence of the graphemes Wāw and ǧīm in this inscription would seem to provide clear evidence that the Thamudic grapheme formed by two concentric circles is not a Wāw as has often been suggested. This second inscription (W. L. 14B/4) reads: *I tm's ḥsr t'ṣf byt wǧy.*

By TM's. He grieved for T'ṢF of the house of WǦY "

The preceding description is repeated in JoAES as follows:

"A longer and philologically more significant Thamudic inscription also found in the Wadi el-Leyyah sheds light on the Thamudic grapheme formed by two concentric circles and disputed as indicating either *wāw* (و) or *ǧīm* (ج). (Pl. XXXVIII):

I tm's ḥsr t'ṣf byt wǧy

لتماس حسر تعصز بيت و جي

By TM's. He grieved for T'ṢF of the house of WǦY" (p. 192B).

"The occurrence of the *wāw* and *ǧīm* in the last name (wǧy) of this inscription is evidence that the grapheme formed by two concentric circles is to be identified as *ǧīm*." " (p. 193A).

Footnote 13 reads as follows:

" " Mr. Michael Macdonald and Professor F. V. Winnett have been kind enough to inspect photographs of this inscription and guide me with their comments on its significance."

because of the general statement dealing with Macdonald's and Winnett's help (cf. above, p. 209) or even because of that affirmation restricted to Macdonald (cf. above, also in p. 209), the testimony of Jobling's footnote 13 on JaT 109 must in good logic (but this is perhaps too much to expect from Jobling) imply a very special, quite unusual assistance of his two mentors in the case at hand; otherwise, it would be an unjustified redundancy.

Jobling's literary production viewed in the light of that general background the details of which remain unavailable, is a vivid illustration of the following biblical axiom: "They are blind men leading blind men; and if one blind man leads another, both will fall into a pit." Here, as in the preceding case, where is the novelty? What Jobling is doing is to repeat, as if it were his discovery, what has been known for a long time already; cf., e.g., HaLiSTI, pl. 26, where two out of the five letters specimens of the letter g are precisely what Jobling is talking about. It is not because G.L. Harding has passed away that Jobling or anybody else has the right to set aside or ignore his correct interpretations and opinions. Furthermore, it is ridiculous to still maintain even as still presently debated questions Winnett's 1937 errors and E. Littmann's 1940 booklet (repeated in the 1966 reprint); cf., respectively, WiSTLI, p. 41:

" wāw—*Wāw* is frequently represented by two concentric circles,"

and Thamūd und Ṣafā, viz. the two concentric circles are either "w" (p. 12) or "m" (p. 17) or "ʿ" (p. 18) beside "g" (p. 9). Littmann, however, who was always determined by his pan-Ar, listed the same two texts, JsaT 207 and 210, as his examples of the double value of "g" and "w" of the two concentric circles. For JsaT 207 and 210, cf. JaTS, pp. 18–19. Moreover, Jobling's argumentation method also is manifestly illogical: he deduces from the fact that the letter follows an ordinary w that it "is to be identified as ǧim." Why could it not be another letter, such as ḍ? Jobling's argumentation does not, and cannot, exclude such a possibility. – Jobling's translation is consistent in giving "TM's," which implies a radical "tm³" + the suffix "s." This is a most ridiculous etymology. Since there is no root such as "tm³," "tmw" or "tmy," Jobling would have to refer to the root mw³ + the preformant t- and the suffix -s. Not only such a dissection is unjustified, but the qtls and tqtls nominal derivations do obviously not exist in

Tham. The name tm'š belongs to the tqtl nominal derivation, which is well-known in Tham. - Jobling reads the sixth letter as "ḥ," the first letter of his verb "ḥsr" which he translates as "to grieve for." The meaning "to grieve for, at" is Ar ḥasira which commands its complement by the preposition ʿalā; cf. LaAEL, p. 688A: حَسِرَ عَلَيْهِ. As all Ar verbs which express or describe feelings have an intransitive signification (cf. also in Saf in JaMAR, XV, p. 125), so, in order to mean what Jobling decided that verb to mean, his "ḥsr" should have been followed by a preposition introducing the complement. Since such a preposition does not exist in the Tham text, Jobling's interpretation is unacceptable. Furthermore, the initial letter is ḳ, not "ḥ." Cf. Ar kasara "to break, defeat" (cf. LaAEL, p. 2610B). - Jobling translates byt as "of the house of." Here again, X byt Y is a formula unknown in Tham. What exists is the verb b(y)t "to spend the night at the abode of" (cf., e.g., JaT 36c) and the 2nd form "to give hospitality to" in JaT 99/2 (cf. above, p. 120).

JaT 109 may be read as follows:

ltm'š ksr tʿṣf byt wgy

By Tam'aš. He has defeated Taʿṣaf [who] had given hospitality to Wāgī.

tʿṣf, cf. ʿṣf of WiTh 55b. - wgy, cf. yygy, imperfect of w/ygy of Hu 531/1.

A most important discovery was released by Y. Shiloh in Eretz-Israel, 19 (1987), p. 290A (cf. also Biblical Archaeology Review, 14, March/April 1988, p. 22B-C) in the form of a sherd with ⊙, the Tham ḡ, accompanied by a wasm dated to the early 6th century B.C. (cf. Biblical Archaeology Review, 14, September/October 1988, p. 66C). This finding confirms the position I have always held on the early date of the NA alphabets.

JoAM 4 (cf. mention of "inscriptions" in p. 470, and photo in pl. 113C) = JoAES 3 (cf. JoAES's mention of three texts in p. 192A, and photo in pl. 36B). Jobling reads the only visible text as " d s ..." in JoAES's caption of pl. 36B. The preceding reading does not even make it certain whether the two letters involved are d, t of the English transliteration, or d, t of the Ar text. Furthermore, the two published photos are very difficult to read. If the two median t's seem assured, the reading of the extreme letters remain impossible, viz. there is not enough ascertained letters to suggest any reading. - The writing process may have involved black ochre, which is well-known in Ḥaḍr rock inscriptions. - Jobling surpasses himself in his careless transliteration of the place name where the text was seen. There are no less than seven different variations of the name: viz.

"Hedeib al Fala" in JoAM, p. 470, and Berytus, 31 (1983), p. 27;
"Hedeib el-Fala" in JoAM, pl. 113C;
"Hedeib el Falā" in Berytus, l.c., pp. 28, 31, 37 and 38;
"Hediebh el Fala" in JoAES, p. 192A;
"Hedeib el Fala" in JoAES, pl. 36B and 37A;
"Hedeib el Falah" in the sketchy map of JoAM, p. 111, and JoAES, p. 196; and
"Hedêb al Fala" in JoPRF, p. 200, which gives the name in Ar letters as حضيبة الفلا, which indicates that none of his seven transliterations is correct!

G - The following inscriptions are not referred to in JoAM; they are mentioned only in JoAES.

JaT 110: the central horizontal senestrograde line of JoAES 1; cf. JoAES, mention in p. 188B without any reading, and photo in pl. 34A. Although Jobling speaks of a single inscription in both places, there are at least three texts. But, the photo is barely legible. - The central horizontal senestrograde line may be read as follows: ʾdmn dwddb ʾAḏaymān, he of Waddab.

ʾdmn, cf. Ar ʾaḍima "iratus fuit" (cf. FrLAL, I, p. 47A). - wddb, e.g., En 245. The name where the inscription comes from is found in four variations, viz.

"ʿAin ʿAbu ʿInsor" in p. 188B and pl. 34A;
"Ain Abu Insor" in the map of p. 186;
"ʿAin abu Insor" in the map of p. 194; and
"ʿAin Abu Insor" in JoPRF, p. 199, where the Ar spelling is given as عين ابو نسور. Beside "ʿ" instead of ʾ of "ʿAbu," where does the "I" and especially the "ʿI" of the first "ʿInsor" come from?

JaT 111 = JoAES 4 (cf. JoAES, p. 192B, and photo in pl. 37A).

Jobling writes that "A tentative transliteration of this inscripuon is as follows: *L ʿṢ M* ..." This transliteration is further repeated in the caption of pl. 37A, as follows: " l ʿṣm " Note that, here again, the dot after "l" is omitted, and the use of the capital letters is introduced. In either transliteration, the last letter of the name is left out by Jobling. The dextrograde text may be read as follows: lʿṣmn By ʿAṣmān. - Cf. JsaT 493.

The peculiarity of this text is that the engraving of ṣ is composed of two vertical overlapping curves) to which a pointed circle was added at the bottom: ⊙

H - The last example retained in this study is JoNAT.

JoNAT of ADAJ, 30 (1986), pp. 261-285, and photos in p. 478-485, publishes what he calls "Selection I" of his "Repertoire" of Tham texts discovered in the ʿAqaba-Maʿan area. But, almost each of his past papers has already given some "selection" of those texts!

1 - The documentation.

a - Facsimiles and photos.

It is to be deeply regretted that Jobling wasted a whole page to each of his facsimiles while joining two photos per page. There is no justification for printing so large facsimiles while some photos are so small, such as p. 479A, B, 481A, 483B, 484A, B, and especially p. 485B. This photo cannot be enlarged properly to assure the reading of all inscriptions and, therefore, it is useless as well as the corresponding pages of the edition, viz. JoNAT 16 in pp. 265B-266A. Similarly, in JoFSA, the photo of his first series in p. 86B is illegible, and there is no photo illustrating his last text (cf. already above, p. 164); consequently, this publication is also useless. His facsimiles, as well as those referred to in the previous pages, are only approximations and wholly unreliable.

b - The origin of the photos.

The photos were taken in two, maybe three, different locations, viz.

2 , his nos. 1 and 8, in "Wadi Judaiyid;"

13, his nos. 2-7, 9, 10, 12-14 and 16 in "Wadi el-Ḫafir," but "Wadi el-Hafir" in his no. 15; and

1, his no. 11, at an unknown place.

The first name is not listed in JoAMS, p. 199B, or in JoPRF, pp. 199-201, or in JoFS, pp. 191-192, or in JoPRS (cf. above D), which deals with Wâdî Ḫafîr.

According to Jobling, the place is located "close to Sad Raqa and south of Jebel el-Jill and north of Quweira" (p. 261B).

The first two names are not mentioned in the aforesaid references; only is "Quweira." But, here again, the spelling is mistreated. It is "Al Quweirah القويرة" in JoPRF, p. 199, but "EL QUWEIRA" in the sketchy map of JoAMS, p. 200 = Berytus, 31 (1983), p. 29, and also of AfO, 29-30 (1983/1984), p. 265. Note that the headings of the four plates of JoAMS are erroneous because they refer to another paper. - For the second name, "Wadi el-Ḫafir," which is a new spelling contradicted by his own transcription of the Ar name, وادي حفير , cf. above, pp. 172-173. Photo no. 1 was taken in 1982; then, follow 5 photos, nos. 2-6, in 1983; then, 10, nos. 7-16, in 1985. Furthermore, the 1985 photos are not listed numerically, viz. [illegible]1B," "99B," "[illegible]B;" etc.; and this fact indicates a random selection. [illegible] 1 of 1985 has the unique siglum of "97B," it is not possible to [illegible] that particular photo was taken.

c - The position of the photos and facsimiles.

The facsimiles of nos. 2 and 8 are rotated 90° counterclockwise and clockwise, respectively; and photo no. 10 is upside down: the checkered band of the scale is above the white one, which bears two small labels, viz. "[illegible]" - the last two units of the year on the left, and an unknown siglum on the right. However, it always remains possible that the scale was placed upside down on the rock.

d - The orientation of the inscribed panels.

There is a man-made mark added to three stones, viz.

"S" below the scale in no. 9 (p. 482A);

"NE" below the scale in no. 14 (p. 484B); and

"N>" above and to the right of the scale of no. 15 (p. 485A).

Each mark doubtlessly indicates the orientation of the panel. Then, the reader finds the following phrases in the commentaries, viz.

"This small panel has a south easterly aspect" [sic!] (p. 261B, no. 1);

"This rock has a south easterly aspect" [sic!] (p. 263B, no. 8);

"This inscription has a southerly aspect" (p. 263B, no. 9);

"This rockface has a south westerly aspect" [sic!] (p. 265A, no. 14); and

"This rock face has an easterly perspective" (p. 265B, no. 16).

Beside the erroneous spelling of the two adjectives of direction and that of "rock face" (cf. also pp. 261B, 262A, twice, 263A, B, twice, 264A, twice, and B, twice), the meaning of those phrases is clarified by the following: "This rockface faces west" (p. 264B, no. 13); and the orientation given to no. 14 is the opposite of that indicated on the photo.

2 - Jobling's epigraphic work.

a - General evaluation.

Here again, Jobling restates a déjà vu. He characterizes his work as "preliminary transliterations, translations and notes" (p. 261A), and he repeats the same in p. 266A as follows: "The transliterations and preliminary translations and notes are tentative and will no doubt need to be revised in the light of new data and further experience in this area of North Arabian."

It is most unwise to deliberately adopt such irresponsible an attitude, but he

...nted to air all easy grudges his papers yearly even doubling and tripling them ...t separate bulletins. It apparently never occurred to him that his papers wou... "counterproductive" by depicting his work as it really is and, consequently, ...hat he would have to spend many years in the future to refurbish his image as ...onscientious worker. Jobling's preceding statements cannot even excuse the re...ease of such incompetent studies which are unjustifiable. The worst, however, ...es in this statement: "While progress in this cataloguing and analytical linguistic and lexicographical research is well under way" (p. 261A).

...ow could such a catalog based on deplorable editions be worth anything? The on...y lasting effect will be to offer an unreliable list of words, such as those ...f HaAIC and WiHaI. Harding, too, built up his list of Saf names before the fi...al edition of the WH texts. Furthermore, how can the reader be sure that all ...orrections that he intends to make will be faithfully reported? The many pages ...f this volume devoted to the detailed analysis of eight of Jobling's papers pro... not only his incompetence but also his superficiality, carelessness and gulli...ility doubtlessly fostered by his determination of gaining favor and support ...rom certain circles.

b - Disrespect of the texts.

...y either carelessness or incompetence Jobling does not seem to res...ct the texts even seen in photos, viz. he invents the letter "n" at the end of ...T 125 a/2, and eliminates JaT 118b, 124c, 126d, the last two letters of JaT ...5c, and the left half of a letter of JaT 113/2.

c - Decipherment of the texts.

His transliterations are full of surprises and follow into the foot...eps of F.V. Winnett's extravaganzas and illogicalities. Of course, not a single ...rd of explanation is volunteered on those anomalies which are not even pointed ...t; and such an attitude leads the reader to believe that the author is not even ...are of them.

(1) As he has done in his last two papers, ...gives ⟨ the value of "g" in his nos. 1/2, 4/1, 8/2 and 16/2 and 6. The com...entary on his 1/2, the first occurrence of that letter in his "Selection," makes ...comment. It is in the commentary on the second occurrence in his 4/1 that the ...mentary has this (p. 262B):

> "The concurrence of lexical with graphic data now suggests that in the 'Aqabe-Ma'an area of Southern Jordan this grapheme represented the Gim phoneme (*G*=ج). [9] "

...ere is the so-called "concurrence of lexical with graphic data"? ... that the reader heard so far is mere statements and more empty affirmations. His note 9 refers to four publications which copy each other, and Jobling does not shrink from giving his own paper JoFRS as his fourth authoritative reference! This paper was discussed above, pp. 172-195. His first and second referred-to papers also have been rebutted in due time. His third reference is R.M. Voight's paper in ADAJ, 28 (1984),"p. 311-314." This paper will be discussed at the end of this section, as (9), in order not to break up my presentation of Jobling's work.

(2) Jobling gives ✗ the value of both "ṣ" in his no. 2 (twice; ṭ being +), and "h" in his 7/3.

(3)Jobling gives ⟨ the value of both "ḏ" in his no. 13/3 in order to accommodate his preconceived idea of finding a divine name, but "ḫ" in his no. 14/2. (4) Twice in his commentary, Jobling uses the same arguments to justify his reading of ⊙ as "d" in his no. 4/3; cf. also his "⊙" of his no. 13/2 (cf. already above, p. 165), and that of ⊞ as "ṣ" in his no. 9; cf. also his no. 14/5.

On the first case he writes that (p. 262B)

> "Professor Winnett has drawn my attention to G. King's proposal that this grapheme represents the phoneme *Ḍad* (ض) in Tabuk.[8] the accumulation of examples of this grapheme in the 'Aqaba-Ma'an survey and research leads me to prefer King's proposal for this grapheme for this area."

Footnote 8 refers to a publication which is "Not yet published" according to a "Mitteilung" of the printer, O. Harrassowitz!

On the second case Jobling states that (pp. 263B-264A)

> "Professor Winnett has drawn my attention to a recent study by Geraldine King in which she advocates that this grapheme should be given the phonetic value of *ṯ* (ث) in Thamudic E.[10] The proposed vocalization of this grapheme would seem to be corroborated by other onomastic and lexical data in the 'Aqaba-Ma'an area and is tentatively accepted here. The root *ḤṮ* is attested in Classical Arabic.[11] "

> [10] G. King, *op. cit.*
> [11] E.W. Lane, *An Arabic-English Dictionary*, 8 vols. London, 1863-1893, p. 2649ff. "

Such an argumentation is wholly unacceptable. "The accumulation of examples of this grapheme" does not, and cannot, prove any theory; the Latin axiom always remains valid, viz. "quantitas non mutat speciem." Moreover, what is the actual va-

..ue of expressions such as "survey and research" and/or "other onomastic and lexical data"? If it comes from Jobling's studies, the pages of this section amply prove his incompetence, carelessness and superficiality. How could he possibly refer to "research," etc and even to his "catalogue of names so far recorded" when he is not even capable of finding out that among the few texts published by himself so far two workers engraved each their text twice, viz. JoAMA, III, 6 and 20 = JaT 106 h and d, respectively (cf. above, pp. 206 and 205), and also JoAMA, III, o and JoNAT 9 = JaT 106o and 120, respectively (cf. above, p.207, and below). However, in Syria, 55 (1983), p. 323B, his commentary on JoPAE 3/2: lwhblh bn ʾdʿn, refers to JoMoFSA 1 published in JoMoFSA, pp. 398-399 and pl.68B = the enlarged photo of JoAMA, II, p. 45, his JoAMA, II, 1, and commentary in pp. 45-46 (cf. already above, p. 190). Both photos make it certain that the letter read "b" by Jobling actually is r, and that Jobling eliminates the letter n of lhn after whr; in fact, Jobling unites in a single inscription two texts engraved, respectively, by father and son, ʾdʿn and hn; cf. JaMAR, XIV, p. 124: lʾdʿn bn whr and lhn bn ʾdʿn. Finally, the last phrase of the above-mentioned quotation and footnote 11 which is repeated in the word list (cf. "see *Lane* p.2649" in p. 267B), do not, and cannot, prove the correctness of his readings. Furthermore, the reader has to check LaAEL to find out that Joblings refers to the geminate verb ltt (cf. already, JaMAR, XIV, p. 24) instead of the weak verb lyt (as in HaAIC, p. 510).

(5) The straight bar | is read "l" in the second name of his no. 1/2, but also "š" in his nos. 4/2, 5, 7/2, 8/1, 11 and 15/3 (cf. above, p. 165). It should be emphasized that in each case | is found with ⌉. Were Jobling logical, he should have read the second name of his no. 1/2 as "ʾsšh;" and had he reproduced the name of his no. 7/2 correctly, he should have found two straight bars with ⌉; thus, he should have read the name as "šʿšh." Furthermore, it should also be noted that the value of "š" is given to) of his no. 13/2, but ⌇ is read "l" in his no. 14/2.

(6) He gives ⌇ of his no. 11 the value of "h;" ⌉ of his no. 14/3 the value of "k;" and ⌉ of his no. 7/1 the value of "ḫ."

(7) He gives the value of d to both ⊢⊣ of his no. 13/3 = I of his nos. 2, 15/2, 16/5 (twice) and T of his no.16/1 (cf. already above, p. 165).

Obviously enough, no palaeographic study or argument is presented in

support of all those jugglings; but such method has already been a characteristic of F.V. Winnett's publications; and, as usual, all those anomalies are thrown at the reader's face as if they were most obvious and in need of no explanation or justification. It is no less obvious that to maintain a double, or a triple, value to several letters is an easy trick to read whatever one chooses to read.

(8) He eliminates the seven dots of his no. 1/1, the left half of the initial letter of his no. 2/2, the last n of his no 11, the last two letters of his no. 14/4, the wasm at the end of his no. 7/1, and the three texts JaT 118b, 124c and 126d. Furthermore, he invents the last letter of his no. 14/6.

(9) R.M. Voigt in ADAJ, 28 (1984), pp. 312A-313A. - The reader remembers that this paper was referred to by Jobling in the case of 𓍢 (cf. above, pp. 218-219). Indeed, Voigt also repeats parrot-fashion Knauf's theories and, therefore, he publishes a system of hypothetical speculations based on erroneous reading and interpretation.

(a) Voigt's first affirmation reads as follows (p.312B):

> "It is a peculiarity of some Southern texts that the verb *wṯm* is used instead of *wgm* "to mourn", which was already noted by G. L. Harding and E. Littmann in their "Some Thamudic Inscriptions"»

His footnote 14 refers to HaLiSTI without any page number; but, the suggestion comes from E. Littmann only (not from G.L. Harding) since it is presented in a note put between brackets on p. 46 in HaLi 494, and all Littmann's suggestions in NA are even at first sight suspicious based, as they are, on his flagrant pan-Ar which is equalled only by A.F.L. Beeston's. Finally, instead of hibernating in his ivory tower with Knauf as companion, Voigt would have been better off to leave a narrow scuttle open so as to keep his files up-to-date. Already 14 years ago, JaMAR, V, p. 101, note 63, which is referred to in JaMAR, XIII, p. 88, and repeated in JaMAR, XIV, p. 21, had rejected Littmann's suggestion on the basis that both wṯm and wgm belong to two different roots and that similarity of expressions does not, and cannot, prove identity of meanings. Indeed, wṯm is referred to Ar waṯama "to break" (cf. FrLAL, IV, p. 435B). But, both Knauf and Voigt are apparently enticed so much by their own tune that they cannot hear anything else.

(b) Voigt's second statement is as follows (p. 312B):

" E.A. Knauf suggested that the letter ḏ, which had so far been accustomed as ḏ, be read in those texts as ġ, cf. e.g., the reading of the verb wgm and the personal names gmr, gmm, grl, gml and gmʾr which could not be interpreted in a straight forward way when reading ḏ instead of ġ."

Knauf's theories were refuted at length in JaNSL, XXV, pp. 17-29, and for ml, cf. above, pp. 187-188. Furthermore, what is the meaning of an expression, such as "interpreted in a straight forward way"? What could be more "straight forward" (if that means "simple" or "direct") than to explain wgm by Ar waǧama, hgn by Ar ḥaǧana or ḥaǧima, ml by Ar malaʾa, or mhr as another example of the well-known nominal derivation composed of two different roots? Knauf's reading of "ḏml" in Ezil 57, to which he should have added Ezil 54 and 108, is inaccurate (cf. JaNSL, XXV, pp. 24-25).

(c) Voigt's third statement is as follows (p. 3123):

"(a) ḏ merged with ṯ, as can be seen from words with etymological ḏ that are written with ṯ, as (TLI 230) ṯʾr, instead of ḏʾr, and (TLI 105) *umṯ*, instead of *umḏ*")"

Although presented as if it were his own discovery, Voigt's footnote 16 refers to Knauf's article which reads as follows (p. 5884-8):

" 2.3.1. ث Steht für /ḏ/ in ثمت *TLI* 156; wenn die aramäische Aussprache dieses nabatäischen Namens gement sein soll, wäre ثمت zu erwarten. ث steht für /ḏ/ in ثر *TLI* 230; ثر "Rache", in ثمث *TLI* 105; 108 (cf. den Stammesnamen "

Here again, it was E. Littmann (not G.L. Harding) who noted " عبدث عبدث " in the edition of Ezil 156 and, as usual, it is a mere affirmation remaining without any kind of proof. Furthermore, Knauf's remark is out of place; Aramaic also knows the root ʿbd. Finally, the name is ʿbdy ʿAbday. What the editors took as a circle is a much thinner horseshoe-like design in the stone which is underlined by a long speck; and these two features have no more epigraphic value than the two equally horseshoe-like designs immediately below. Whenever he can, Knauf always mentions all the references he knows. Blinded by Littmann's note, Knauf fails to inform his reader, or he was unaware of, that the name "ʿbdt" was also a part of Milik's reading (p. 488) of Hamm 24. This text may be read as follows:

ʾʿbd m ṯʿr By ʿAbd. He has crushed Thʿr. – ʿbd, e.g., JaT 33c. – m, e.g., Hat 511. – ṯʿr, cf. Ar taʿara "vociferatus fuit" (cf. Fränkel, I. 1923).

In Ezil 28C, here again it was E. Littmann who suggested an extravagant interpretation involving a curse, "Cause blood revenge!," addressed to an unspecified deity against an unnamed person!!! The interpretation is introduced by "Probably"

not contains " ظ ظ ظ " Littmann's gratuitous emendation is evidenced by the root: taʾara "to look, scarcely, or intently at someone" (cf. Lane, p. 2721). Furthermore, the name "ʿbdt" does not exist in Ezil 28C (cf. Jamme, PEQ, p. 129, note 29).

In Ezil 105 and 108: here again, it was E. Littmann who suggested "ʿbdml(t)". The name ʿbdml belongs to the well-known derivation ʿbdʾl from the root ʾdl. The name "ʿbdml" of Saf Al 633 must be corrected to ʿbdʾl (ʿAbdil; cf. Ar ʿābil "tailor" (cf. Lane, p. 8073).

(d) Voigt's fourth statement is as follows (p. 3123):

[1] "(b) Now if ḏ did merge with ḏ in the same way, we may expect to find some instances of ḏ for etymological ḏ. There are, however, only some cases of ḏ for ḏ, as in (TLI 490) rḏ, instead of rḏ, and (TLI 69) yḏr, instead of yḏr." [3]

Here again, although presented as if it were his own discovery, Voigt's footnote 17 refers to the place where he copied his ideas, viz. Knauf's article which is reproduced here (p. 5883):

" 2.3.2. Auch scheint in den Inschriften dieses Gebietes /ḏ/ mit /ḏ/ zusammengefallen zu sein. ث steht für /ḏ/ in ثر *TLI* 490; in ثر *TLI* 69; in ثر *TLI* 255 (ثر, ثر und ثر sind gut belegt)."

Voigt does not give the reason why he excluded Knauf's third example.

Ad [1] – Voigt's statement is a vicious circle and does not contain any reason, not even a lame excuse, to render his hypothetical claim possible and, therefore, admissible.

Ad [3] – The two examples given by Knauf and Voigt are also taken from Hölbl, p. 175. For more details, cf. my study of Hölbl's paper in the following chapter.

(e) Voigt's fifth statement is as follows (pp. 3123-3134):

"The rare instances of written ḏ, as in (TLI 59) wḏ, (TLI 198) ṣḏḏ and (TLI 894) *hdn*", should then be seen as cases of historical orthography."

This statement is unintelligible since there is no ḏ in any of these three personal names, and the expression "cases of historical orthography" has a nice sound and, according to Voigt himself on p. 3133,

"Historical orthography is based on the assumption that characters which are widely used tend to be preserved."

In fact, it is nothing but a "latina hoc" of a mere tendency based on a mere "assumption."

Footnote 18 reads as follows:

" = Cf. the remark in *TLJ* 494 that *d* has here a "curious form" that may be accounted for by the fact that it is rarely used "

For Saf Hali 494, cf. JaNAR, V, pp. 100-101, note 63. - Harding explains that "curious form" as follows: "494 has the loop on the wrong side of the vertical stroke" (cf. HaLiSTI, p. 3). This is quite temerary a judgment because it implies that he, Harding, dares to tell a Tham writer that his engraving of the letter d is wrong, . What can be the difference between and ? Next to engraving letters in the direction opposite to that of the text, those examples are other proofs of the liberty which Tham writers enjoyed in the engraving of the letters since all elements of each letter are present. In another case, which is more remarkable than the present one, Harding states this: "d. Two main types with many variations" (p. 3). Which are those "two main types"? If there are "main types," there must be "secondary types." Which are they? In fact, there are four "main types" according to my schema of next page, and the most important of them is C. A 1, 3 and 16 are differenciated by the number of semi-circles crossing the central vertical staff, 1, 2 and 3, respectively. Apparently, Harding found nothing "curious" in D16 when compared with B1.

(7) Voigt's sixth statement is as follows (p. 313A):

"the following personal names containing *d* instead of etymological *z*:

(WIJ 349) *cdmn.* with root *czm.*
(WIJ 352) *dnt.* with root *znn.*"

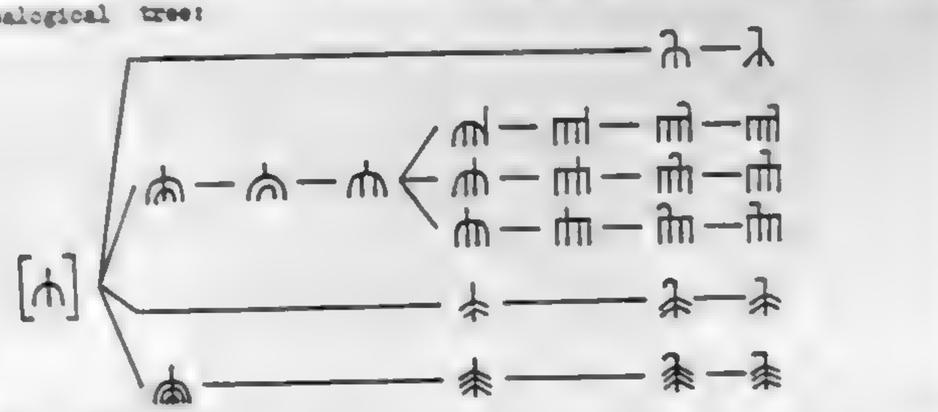
The siglum "WIJ" is used twice erroneously instead of "TIJ." Furthermore, the roots ʿdm and ʿẓm are still used in the Middle-East Ar; cf. ʿdm, 2nd form "devenir dur (comme de l'os)" and ʿẓm "il a été peiné" (cf. C. Denizeau, Dictionnaire des parlers arabes de Syrie, Liban et Palestine, Paris, 1960, pp. 335 and 336, respectively; cf. also A. Barthélemy, Dictionnaire arabe-français, Paris, 1942, pp. 534 et 537, respectively); cf. also LaAEL who states that "ʿaẓm is a dial(ectic) var(iant) thereof," viz. of ʿaḍm (p. 2075B). Finally, the roots ḏnn and ẓnn are distinct even in Ar; cf. Ar ḏanna "to be, become, niggardly, tenacious" and ḏinnat "niggardliness, tenaciousness" (cf. LaAEL, pp. 1805C and 1806A, respectively); and ẓanna "to think, opine, suppose" and ẓinnat "suspicion, evil opinion" (cf. LaAEL, pp. 1924B and 1925A). - WH 349 is completely mistreated in WiHaI (p. 99, and facsimile in p. 667); the text may be read as follows: lgrmn bn hnznn hdr To Garmân, son of Hanaznân [belongs] the dwelling-place." - grmn, e.g., LP 558. - hnznn, cf. Ar hanîza "(something) to become stinking" (cf. LaAEL, p. 815B). Win-

The letter d in HaLiSTI's pl. 1-26.

	A	B from A	C from B	D Intermediate forms	E from B	F from C
1	[ ]	(506)			(504) (507)	
2					(500)	
3	(368)	(4)	(76)	(379)	(170)	
4		(489)	(92)	(156)	(392)	
5		(223,391)	(519)	(171)	(369)	(189)
6		(490A)	(520)	(109)		
7		(481)	(584)	(588)		
8		(65)	(455)	(5)		
9		(490B)	(173)	(7)		
10			(225)	(10)		
11			(508)	(411)		
12			(505)	(299)		
13			(34)		(59)	
14			(174)			
15			(255)			
16	[ ]			(45)		

The numbers of the texts in the edition are given between parentheses. HaLiSTI's 12 letters in his pl. 26 are those of nos. 3 = my D8, 7 = my D9, 45 = D16, 58A = my C7, 58B = my D7; 65 = my B8; 76 = my C3, 223 = my B5, 299 = my D12, 392 = my E4, 455 = my C8, and 500 = E2; thus 5 from D, 3 from C, and 2 from both B and

, thus. An assortment of examples of derivations from four types. – The type
…ch Aʾ derives is the ordinary ḥ and will never be found as such becaus
: would imply the bivalence of ḥ. Types A3 and 16 are derived from [ḥ] with
double or triple loop, while types C, E and F are developments from B, 3 and
, respectively. – The preceding letter forms may be schematized in the follow-
ng genealogical tree:



hett's reading of "ʾʿ(ǧ)n" instead of hnznn is nonsensical, and neither p. 99 nor
. 533 explains the name which is vocalized "ʾAʿ(ǧʾ)ān" and "ʾAʿ(z)an," respecti-
ely; both spellings eliminate the only root which might explain the name, viz.
ʿ.

(g) Voigt's seventh and last af-
firmation is as follows (p. 313A):

> "In other personal names, it is not quite clear whether South Safaitic *d* represents North Safaitic *d* or *z*, as it may be in (WLJ 58) *gd*, for which different roots can be given."

Here again, "WLJ" is an error instead of "TLJ." Furthermore, instead of his ten-
entious, vague statement, Voigt should have informed his reader that there is
practically only one root to explain the name ġḏ, viz. Ar ġaḍḍa "to lower one's
eyes" (cf. LaAEL, p. 2264A), as Ar ġāḍa (i) means "(water) to become scanty" (cf.
LaAEL, p. 2316C). To consider the possibility of Ar ġāẓa (i) "to affect (someone)
with anger, wrath, or rage" (cf. LaAEL, p. 2317C) is looking for difficulties whe-
re there are none.

Here again, Voigt's three examples of his 6th and 7th statements are al-
ready listed in LiMAD, p. 171, but Littmann's presentation is much more elabora-
te and much more detailed than Voigt's sketchy paper, and Voigt does not even re-
fer to Littmann.

…summarizes Littmann. Although the first to invent these theories and to pub-
lish them in detail in LiMAD, Littmann does neither get the credit for it nor e-
ven have his name printed in a footnote. Furthermore, the origin of all these …
sonantic equations came from the fancy par-ar of Littmann who every time gave …
mere statement without ever producing a reason or even a lame excuse. Littmann's
extravaganzas are repeated parrot-fashion and amplified by persons who do not e-
ven acknowledge their plagiarism and build up their papers on erroneous reading
or interpretation.

d – Interpretations and commentary.

Jobling's interpretations offer the reader surprising anomalies,
and two of them, (4) and (6), are extravagant novelties.

(1) In his no. 3 (p. 262A), his name "…," although not impossible, is not necessary.

(2) In his no. 7/1 (p. 263A), the text would remain incomplete. (3) In his no. 12 (p. 263B), he gratuitously isolates the inital w of the text. (4) In his no. 13 (p. 264B), he publishes an unjustified novelty, which can hardly be equalled. He isolates the letter "ẓ" in his transliteration, and that "r" becomes "rʾ" in his "Lexicon" (p. 266A), which in turn is translated as "see;" thus relating his "r" to the Ar verb raʾā (i). His "Lexicon" has this: "*R*. CIS 1338; CIS 5263; *Clark*, p. 300. *Wright*, p. 374; *Thatcher*, p. 186."

(a) Although it is "ẓ" that he has to explain, his first three references deal only with "rʾ," which does not exist in the text; and they all are Saf, not Tham.
In CIS 1338, the letter y must be restored in brʾ[y] because this expression is too well-known; cf. AION, 31 (1971), pp. 34-37, where it is stated that "the letter y can easily have been forgotten by haplography with the preceding ʾ."
In CIS 5263, which belongs to CIS 5262+5263, it is E. Grimme who erroneously read "rʾ" (cf. AfO, 8 [1932/1933], p. 110; and his reading was endorsed by CIS (p. 645B); the two letters read bʾ; cf. also AION, l.c.
"Clark, p. 300" = CI 559, where "rʾ" does not exist; cf. JaMAR, XXXI, p. 64.
Of course, Jobling could not care less about previous studies, but ... his three references are erroneous.

(b) The second half of his foot-

note refers to W. Wright's and G.W. Thatcher's grammars.

On his first reference, vol. II, p. 374, Jobling fails to inform his reader that Wright deals with "POETIC LICENSES."

The second reference gives the conjugation of the Ar verb ى ر ء. Jobling did not have to refer to Thatcher's grammar since the same information may be found in Wright's, I, p. 93. And Jobling certainly was very happy to see the masculine singular imperative ر . Unfortunately, Jobling does not even attempt to justify his reference to, and his choice of, the Ar verb r'y. It most probably never occurred to him that the two Ar verbs r'y and wry have the imperative as r, ر and ر, respectively. Cf. Ar wará (i) "affecit laesitve in pulmone aliquem" (cf. FrLAL, IV, p. 460A). As for his two references, it is both irrational and anachronistic to project the Ar "poetic licenses" and/or the peculiarities of the Ar conjugations into NA.

(c) Jobling's translation of his l.3 reads as follows: " 3. And Oh Dushares see (the) goats " (p. 264B). What a strange request to formulate to a deity! It is the same as a boy telling his friend: "John, look at my toy." Jobling's decipherment is wholly unacceptable. Furthermore, the commentary makes it even worse because it shows a lack of basic good common sense; it reads as follows (pp. 264B-265A):

> "The prayer to Dushares on this rock-face may be related to the rock drawings which depict a hunting scene in which a long horned caprid has been pierced by an arrow."

Jobling's reading of "m'z," which is presently attested in Saf only, is erroneous. Furthermore, Saf m'z always refers to the goats belonging to the author of the text. Therefore, Jobling's connection of his "m'z" with the transpierced ibex is contradicted by all epigraphic material published so far. Finally, to connect the hunted animal with his so-called "prayer to Dushares" is simply ridiculous.

(5) In his no. 14/2 (p. 265A), his text begins with "By the son of."

(6) In his no. 15/3 (p. 265A), the second novelty of his paper is the invention of the 2nd person of the direct speach; viz. "sm't" is understood as "May you hear" (p. 265B); and to make his translation more palatable, he invents an inexistent invocative particle, "h-," which he translates as "O;" note that he also spells as "Oh" in his no. 13/3 (cf. above).

(7) Jobling confuses singular and plural (cf. already above, p. 161), viz.

p. 263A, l. 13 of the commentary on his no. 6 = JaT 117, "is" must be corrected to "are;"

p. 263A, ll. 5-6 of the commentary on his no. 7 = JaT 118, "features" must be corrected to "feature;" and

p. 265B, l. 8 of the commentary on his no. 15 = JaT 126, "are" must be corrected to "is."

(8) The inconsistency of Jobling's working method is also illustrated by the two following facts.

In his no. 14 (p. 265A), he gives the no. 4 to a line which he connects with the preceding line, no.3; but

in his no. 16 (p. 265B), he does not give any number to his 5th line which he connects with his not. 4.

e - The word list (pp. 266A-267B).

Jobling had to establish the word list of his small material which, according to him, is composed of 38 short texts. Therefore, the reader had expected to find it accurate and complete. It is neither.

(1) - The word list is marred with the too frequent absence of diacritical signs, which also plagues the edition of the texts itself.

(2) - Jobling does not seem to know that the basic purpose of a word list is to primarily indicate the reference(s) to each word. Here again, the reader is frustrated: he has to add the references himself.

(3) - Jobling's word list with 68 entries contains only the names of publications where each word or its related form(s) may be found.

(4) - He refers 50 times to HaAIC, but does not inform his reader whether HaAIC lists any Tham text. As a matter of fact, in 27 cases HaAIC does not list a single Tham reference. Yet, Jobling reserves his notes "possible new name" (seven times) = "possibly a new name" (once) to names which he did not find in HaAIC either as such or under any of their related forms, with the only exception of "mb'ln."

(5) - There is no way of knowing how Jobling's mind is going to react in any particular case. Here follow a few examples.

(a) "ZR'LH see ZR'. HIn p.297" (p. 266B).

HaAIC refers only to Tham WiAM T 13. But, the name is lb' (cf. JaMAR, XV, p. 37).

(b) " SR HIn p.814-815" (p.266B).

The two numbers should be corrected to 314-315.

(c) " DBN (or WBN. see WBNHN WH p.621

DR (or WR. see WH p.622"

He reads those names, respectively, in his nos. 13/2 and 4/3. In the commentary on the latter (p. 263B), he speaks only of two possibilities of reading his ⊙ and ⊙, viz. "j" or "ḏ" (cf. already above, p. 219); the commentary on his no. 13 does not say a word on the question. Therefore, where does the "W" come from? Where else except from his fantasy? Furthermore, since he retains "ḏBN" and "ḏR," why does he not refer them both to ḏb and ḏr in HaAIC, pp. 380 and 382, respectively? Finally, "WBNHN" is an error instead of "WBNḪN," and this name does not exist (cf. JaMAR, II, p. 96).

(d) "RBQT see RBQN - RBQ Hln. p. 267" (p. 266B). Jobling copied two consecutive names of HaAIC, but he miscopied the second which is "rbk."

(e) "Š'N'T see Š'N'. WH. p. 587" (p. 267A). HaAIC correctly lists šnʾt in CIS 3065 (p. 359).

(f) "Š'LH Hln. p. 351" (p. 267A). HaAIC does not list "šʿlh" at all but only the two Saf names šʿl and šʿlt. The only Tham reference to "šʿlt" is JsaT 657, but HaAIC also lists the same name under "ʿlt" (p. 430), without any cross-reference in those places. The name of JsaT 657 reads rnʿlt (cf. JaMAR, IV, p. 134, note 62).

f - The "Lexicon" contains 7 entries only (pp. 266).

(1) - The second entry is "SM'. WH p. 639" (p. 266A). "smʿ" does not exist in his 15/3, but "smʿt," which is interpreted as a second person (cf. above, p. 228).

(2) - The third entry, "R'." does not exist; cf. above, p. 228.

(3) - Three entries are missing, viz. h-, the invocative particle in his no. 13/2; -h, the personal pronoun in his no. 15/3; and kr, fem. dkrt, verb, in his no. 16/4.

g - The "Index of Proper Names" (pp. 266B-267B).

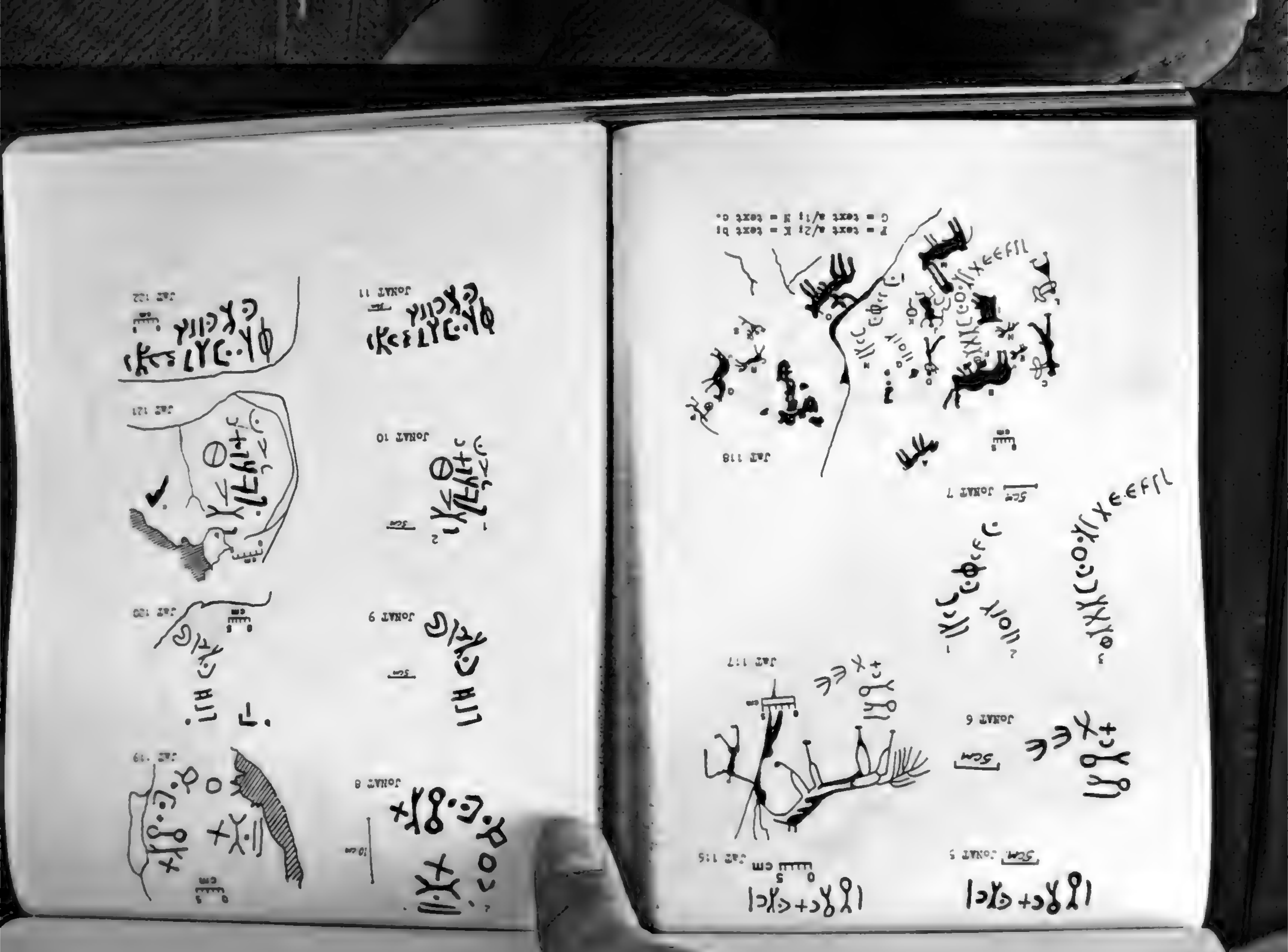
Three entries are missing, viz. "[s]ʿdn" of his no. 8/2; "z..." of his no. 16/1; and "ngʾt" of his no. 8/2.

3 - The first fifteen stones: JaT 112-126.

JaT 112 = JoNAT 1 (cf. p. 261B, facsimile in p. 268, and photo in p. 478A).

a = no. 1: in the upper left side of the panel.

lʿbd By ʿAbd. - As in the edition; cf. JaT 33c. To the left and below the text, there is a series of seven dots isolating the text and the ibex from text b. Such a series of dots is well-known in Saf; cf. also, however, the five



JONAT 12

JAT 123

JONAT 13

JAT 124

JONAT 13

JONAT 14

JAT 125

JONAT 15

JAT 126

dots of Hu 813. Neither those dots nor the ibex are mentioned by Jobling in his commentary or represented in his facsimile. Therefore, he does not justify to which text he connects the drawing of the animal.

b = no. 2: to the right of and below the ibex.

lntn bn ʾslh By Nâtin, son of ʾAsluh.

ntn, cf. Ar natana "turpia et obscoena locutus fuit" (cf. FrLAL, IV, p. 240A). - ʾslh, as in the edition. HaAIC (p. 45, to which Jobling refers in p. 266B) relates the name to the root "ʾs" = either ʾss or ʾws (p. 40); thus , ʾs + lh. This interpretation is based on classical Ar vocalization; in Tham, however, the qtl+h formation is attested almost thrice as often as the qtl+lh derivation.

Ja 113 = JoNAT 2 (cf. p. 261B, facsimile in p. 269, and photo in p. 478B).

lrnh bn ḏḏʾl ḏʾl 1 By Rânih, son of Ḏawdʾil, he of the tribe

ʿwmh 2 Wamah.

rnh, cf. Saf CIS 1150. - ḏḏʾl, cf. ḏdlh of JsaT 105d. - ḏʾl, as in the edition. wmh, cf. Ar wamhat "lentigo" (cf. FrLAL, IV, p. 507B).

JaT 114 = JoNAT 3 (cf. pp. 261B-262A, facsimile in p. 270, and photo in p. 479A).

lmr bn ʾṭ ḥd By Marr, son ʾAṭṭ. He has made [these] marks.

Same reading as in the edition, where "ʾthd" becomes doubtful and a "possible new name" in p. 266B. - mr, e.g., Hamm 16. - ʾṭ, cf. Saf ʾṭw, name of a person in WH 2122, and of a tribe in CIAS 1011. - ḥd, cf. hadda "to mark, impress (a thing); make a mark or marks, or an impression or impressions (upon a thing)" (cf. LaAEL, p. 705C). The verb ḥd refers to both the text itself and the camel (not "probably," as in Jobling's commentary) to the left; both have the same patina. Finally, Jobling writes (p. 262A) that

> "The shape of the antepenultimate grapheme may also be read as *H* in Thamudic C and D according to Winnett's taxonomy.[4]"
>
> "4 F.V. Winnett. *A Study of Libyanite and Thamudic Inscriptions*. Toronto. 1937. Plate X"

The preceding statement is absolutely untrue: that very letter is given in WiSLTI as h in his "B," "C," "E," doubtfully in his "A," and even as the sole letter in his "D." It is NOT to be found under ḥ. In the present case, there is no text to read and to understand, but only letter forms to look at ... and Jobling appears to be unable to do even such simple, basic operation correctly. Furthermore, the roots "thd" or "ṯhd" do not exist.

JaT 115 = JoNAT 4 (cf. pp. 262A-B, facsimile in p. 271, and photo in p. 479B).

a = no. 1: on the extreme left.

ltrm By Tarîm.

Cf., e.g., Rbo 9. All references listed by Jobling are those of lrm; they are obviously irrelevant to prove his reading.

b = no. 2: immediately to the right of, and parallel to, the preceding text.

lʾrl bn flṭ By ʾAryil, son of Paliṭ.

ʾrl, JaT 106v. - flṭ, as in the edition; Hu 279.

c = no. 3: immediately to the right of, and parallel to, the beginning of the preceding text.

lgr By Gâr. - Cf. JaT 106i.

d = no. 4: immediately to the right of, and almost parallel to, the preceding text.

lnhm bn dql By Nihm, son of Dâqil.

As in the edition. - nhm, cf. Saf, e.g., JaS 51. - dql, e.g., HaLi 278.

JaT 116 = JoNAT 5 (cf. p. 263B, facsimile in p. 272, and photo in p. 480A).

lṣṣrt bn ʾrl By Ṣaṣrat, son of ʾAryil.

ṣṣrt, as in the edition. The root ṣṣr is attested in Middle-East Ar ṣaṣara "émaner, être édicté (ordre)" and 2nd form "occuper la place d'honneur" (cf. A. Barthélemy, Dictionnaire arabe-français, Paris, 2nd fasc., 1936, p. 432). Jobling introduces his commentary on the text by repeating in advance a note which is formulated in the commentary on the following text. Ad quid? Furthermore, he has already read "ʾrs" instead of ʾrl in JaT 115b, but has not made any comment at all on the name. It is in the present commentary that he writes this (p. 262B):

> "In this particular case the patronym *'RŠ* is also given. The proper name *'RŠ* is well attested in North Arabian (see *HIn* p. 37)."

The reference involved, which is repeated once more in p. 266B, is invalid to prove his reading but shows that, following into F.V. Winnett's footsteps, he seeks to find well-known names at the expense of palaeography.

JaT 117 = JoNAT 6 (cf. p. 263A, facsimile in p. 273, and photo in pl. 480B).

lṣṣrt 1 By Ṣaṣrat.

ḥṭṭ 2 He has cut [this].

The text runs below the hindquarters of the horse. - Although ṣṣrt is already attested in the preceding text, it is only here that Jobling states that (p. 263A)

> "The proper name *ṢṢRT* is new," which, surprisingly enough, becomes (p. 267A) "possible new name." Jobling also speaks of a " well preserved drawing of a bridled horse and owner."

The author does not specify that the horseman is leading his mount. Furthermore,

t is visible even at first sight that the horse is mostly outlined with some fil
ings on the withers, mane, belly and hindleg; the rest of the picture is blank. It
s, therefore, regrettable that Jobling did not remember his erroneous comment on
aT 106 (cf. above, pp. 176 and 180). Finally, Jobling states (p. 263A) that

[A] "The curious procedure of inscribing the second grapheme in reverse also occurs frequently with this signature and may be an ideosyncracy of script consciously employed by a particular individual or artist. Preliminary study of the several panels to which this name and the formula SSRT HTT is affixed suggests a common style and method of drawing." [B]

d [A] - Such a method of engraving is not in the least "curious" for anyone who is aware of the complete liberty which which Tham, as well as Saf, wrote their texts either vertically (in both directions) or horizontally (in both directions) or as boustrophedon or in serpentine of any kind, etc. If so, they were t liberty to write the letters they way they choose or prefer ... and there is o "ideosyncracy" [sic!], a word commonly used as synonymous of "eccentricity," in making full use of their own particular style; beside the examples given above (p. 224), cf. ⌉ ⌊ of JaT 118, end , ⌐¬ of JaT 120, beginning; and with the exception of JaT 112a, each letter d of the texts republished here (JaT 113, 114, 115d, 119, 126 a, b, twice) is written in the direction opposite to that of the text. Finally, the identity of author of both JaT 116 and 117 is not proven, as Jobling claims, by the engraving of the second letter upside down in relation with the first. This is a part of the proof, but not the most important one. This, indeed, could happen in two different ways, viz. 𓎛𓏏𓏏 or 𓎛𓏏𓏏. The identity of author is proven by the fact that the second alone is used.

Ad [B] - If it is easy to understand what Jobling had in mind, it is less clear that his wording, which contains an erroneous "is" instead of are" (cf. already above, pp. 228-229) is incomprehensible because "SSRT HTT" cannot be "affixed" to either ssrt or "several panels." Furthermore, the adjective "common" is an erroneous latius hos since those texts are manifestly by the same person. JaT 118 = JoMAT 7 (cf. p. 263, facsimile in p. 274, and photo in p. 418A).

The five sections of the photo are composed of 19 elements.

I: the hunting scene of the upper left section:
- A: a dog barking at the hunted beast B;
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II: a second hunting scene in the lower left section, viz.
- I: an unspecified hunted beast;
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- K: text b by archer J, which is eliminated by Jobling; and
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III: the bottom left section: M: an oryx.

IV: the central section: N: text c with the name of O; and
O: a man mounting an undetermined animal.

V: the right section:
- P: a beater facing the beast with a shield in his left hand and a bat in his right;
- Q: an undetermined hunted beast; note the small blank between the hindlegs;
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- S: an archer below and behind the beast in the position of shooting.

Jobling's commentary is another example of a messy confusion. He divides the whole representation into two panels which he describes as follows. His first section is this (p. 263A-B):

[A] "The central panel and focus to which the inscriptions seem to relate features a hunting scene. In this scene the object of the hunt appears to have been a bovid. This animal has a humped back. A later artist has clumsily added horns more appropriate to an ibex or caprid. Dogs and hunters appear to be moving in for the kill and several darts and arrows are depicted in flight. The figure next to the second inscription may be the representation of a woman. Thus the scene preserves details of an important aspect in the life of the North Arabian indigenes and may also indicate that hunting was not a role restricted to any one gender." [B] [C] [D] [E]

and his second section (p. 263B):

[F] "In the second panel on the bottom right hand side of this rock face there is preserved the scene involving a horse-like animal. The human figure to the left and behind the horse is armed with a bow and may well represent the stylized form of a woman. The human figure in front of the horse is armed with a round shield and is brandishing what appears to be a sword which has a straight blade. The well drawn dog in the top left hand corner would appear to be assisting by holding the horse at bay. The patronym in the second inscription is difficult to read because of later hammerings which have weathered away the patina: the ductus of the middle bar of the final grapheme is difficult to discern." [G] [H] [I] [J]

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and his second section (p. 263B):

[F] "In the second panel on the bottom right hand side of this rock face there is preserved the scene involving a horse-like animal. The human figure to the left and behind the horse is armed with a bow and may well represent the stylized form of a woman. The human figure in front of the horse is armed with a round shield and is brandishing what appears to be a sword which has a straight blade. The well drawn dog in the top left hand corner would appear to be assisting by holding the horse at bay. The patronym in the second inscription is difficult to read because of later hammerings which have weathered away the patina: the ductus of the middle bar of the final grapheme is difficult to discern." [G] [H] [I] [J]

It is visible even at first sight that the horse is mostly outlined with some fillings on the withers, mane, belly and hindleg; the rest of the picture is blank. It is, therefore, regrettable that Jobling did not remember his erroneous comment on JaT 106 (cf. above, pp. 176 and 180). Finally, Jobling states (p. 263A) that

[A] "The curious procedure of inscribing the second grapheme in reverse also occurs frequently with this signature and may be an ideosyncracy of script consciously employed by a particular individual or artist. Preliminary study of the several panels to which this name and the formula *SSRT HTT* is affixed suggests a common style and method of drawing." [B]

Ad [A] – Such a method of engraving is not in the least "curious" for anyone who is aware of the complete liberty which which Tham, as well as Saf, wrote their texts either vertically (in both directions) or horizontally (in both directions) or as boustrophedon or in serpentine of any kind, etc. If so, they were at liberty to write the letters they way they choose or prefer ... and there is no "ideosyncracy" [sic!], a word commonly used as synonymous of "eccentricity," in making full use of their own particular style; beside the examples given above (p. 224), cf. ⌉⌋ of JaT 118, end, ⌐¬ of JaT 120, beginning; and with the exception of JaT 112a, each letter d of the texts republished here (JaT 113, 114, 115d, 119, 126 a, b, twice) is written in the direction opposite to that of the text. Finally, the identity of author of both JaT 116 and 117 is not proven, as Jobling claims, by the engraving of the second letter upside down in relation with the first. This is a part of the proof, but not the most important one. This, indeed, could happen in two different ways, viz. [illegible] or [illegible]. The identity of author is proven by the fact that the second alone is used.

Ad [B] – If it is easy to understand what Jobling had in mind, it is less clear that his wording, which contains an erroneous "is" instead of are" (cf. already above, pp. 228–229) is incomprehensible because "SSRT HTT" cannot be "affixed" to either ssrt or "several panels." Furthermore, the adjective "common" is an erroneous latius hos since those texts are manifestly by the same person.

JaT 118 = JoNAT 7 (cf. p. 263, facsimile in p. 274, and photo in p. 418A).

The five sections of the photo are composed of 19 elements.

I: the hunting scene of the upper left section:
- A: a dog barking at the hunted beast B;
- B: the hunted, heavy-bodied animal which is looking rightward and seems to be stillstanding;
- C: an archer behind the beast and in the position of shooting;
- D: another archer facing the beast;
- E: a dog closing in behind the beast;
- F: text a/2;
- G: text a/1, mentioning the name of archer D; and
- H: another dog running parallelly to dog E.

II: a second hunting scene in the lower left section, viz.
- I: an unspecified hunted beast;
- J: an archer facing and aiming at the beast;
- K: text b by archer J, which is eliminated by Jobling; and
- L: another archer behind the beast and aiming at it.

III: the bottom left section: M: an oryx.

IV: the central section: N: text c with the name of O; and
O: a man mounting an undetermined animal.

V: the right section:
- P: a beater facing the beast with a shield in his left hand and a bat in his right;
- Q: an undetermined hunted beast; note the small blank between the hindlegs;
- R: a dog closing in behind the beast; and
- S: an archer below and behind the beast in the position of shooting.

Jobling's commentary is another example of a messy confusion. He divides the whole representation into two panels which he describes as follows. His first section is this (p. 263A–B):

[A] "The central panel and focus to which the inscriptions seem to relate features a hunting scene. In this scene the object of the hunt appears to have been a bovid. This animal has a humped back. A later artist has clumsily added horns more appropriate to an ibex or caprid. Dogs and hunters appear to be moving in for the kill and several darts and arrows are depicted in flight. The figure next to the second inscription may be the representation of a woman. Thus the scene preserves details of an important aspect in the life of the North Arabian indigenes and may also indicate that hunting was not a role restricted to any one gender." [B] [C] [D] [E]

and his second section (p. 263B):

[F] "In the second panel on the bottom right hand side of this rock face there is preserved the scene involving a horse-like animal. The human figure to the left and behind the horse is armed with a bow and may well represent the stylized form of a woman. The human figure in front of the horse is armed with a round shield and is brandishing what appears to be a sword which has a straight blade. The well drawn dog in the top left hand corner would appear to be assisting by holding the horse at bay. The patronym in the second inscription is difficult to read because of later hammerings which have weathered away the patina. the ductus of the middle bar of the final grapheme is difficult to discern." [G] [H] [I] [J]

Ad [A] – Here again, the singular "features" instead of the plural. Furthermore, his "central panel" is composed of four sections, my I–IV.

Ad [B] – He deals only with my section I. Contrary to Jobling's statement, animal B does not have "a humped back;" it is both its rump and shoulders that are humped. Finally, the first substantive of the expression "an ibex or caprid" is superfluous because an ibex is a number of the caprid species.

Ad [C] – Here again, he fails to specify how many humans and dogs there are.

Ad [D] – This statement is erroneous: Jobling dreamed about them because they do not exist.

Ad [E] – He speaks of my D. Here again, the upper half of the body is simply outlined; cf. already above, JaT 102 (p. 180). His erroneous deduction is but a repeat of his commentary on JaT 102.

Ad [F] – The animal Q can hardly be "a horse-like animal." But, it simply becomes "the horse" in his J.

Ad [G] – Here again, cf. my remarks above, on E.

Ad [H] – There is no question of a "sword" since the man only has defensive weapons of which one is a shield.

Ad [I] – The dog is not "holding the horse at bay;" it is about to jump on the hindquarters of the beast.

Ad [J] – Because his note begins with the mention of "the second inscription," Jobling's simplistic mind thought it all natural to add it to the paragraph dealing with "the second panel." In fact, he is talking about his "Ḥ̇" of "QRḤ̇" of his inscription no. 1, which is a part of his "central panel." Finally, his last phrase is erroneous: there is neither "middle bar" nor "hammerings" visible there in the photo.

a = G+F = nos. 2+3; l.1 is aslant to the right of D; l. 2 is below the belly of B, to the right of I, and the lower half curves leftward because of M.

llʿlh	1	By Ilʿālih,
wʾḫʾb bn gnʾl ḥṭṭ kll	2	and ʾAḫʾab, son of Gawnʾil, has cut all.

lʿlh, WiAM 12. – ʾḫʾb, as in the edition; cf. Liḥ JsaL 12. – gnʾl, cf. Green (5). – ḥṭṭ kll, as in the edition; kll refers to the whole hunting scene, including the two lines of the text.

b = K = a dextrograde text making a concave line which is cut into two sections by J; eliminated by Jobling.

→ lby Labīy. – Cf. HaLi 299; name of archer J.

c = N = no. 1: to the right of G, K, J, and the horns of M.

lhrb	1	By Hārib,
bn qr lbn Wasm	2	son of Qarr. He was struck with violence. Wasm.

hrb, as in the edition, which also lists "ḤRB" in p. 266B, where the spelling "ḫrb" is authenticated by his reference to "*Hln.* p.182." In HaAIC, all names beginning with "h" are listed on pp. 605–632. Cf., e.g., HaLi 461. – qr: Hu 501 /2. – lbn, as in JaT 106 c/1. Jobling reads the end of the text as "*QRḤ̇ BN QRḤ̇* son of ... ". The hypothesis of a text being incomplete is a trick demonstrating incompetence, as shown in JaSN, pp. 99–107. Furthermore, the twice repeated "QRḤ̇" of p. 263A becomes "QRḤ" in p. 267A. Moreover, Jobling's facsimile fails to include the wasm, a vertical line, to the left of the last two letters of the text; cf., e.g., JaT 105d and reference.

JaT 119 = JoNAT 8/1+2 (cf. p. 263B, facsimile in p. 275, and photo in p. 481B).

L. 1–2 are slightly aslant, dextrograde and parallel to each other; l.3 is a concave line engraved vertically and to be read from bottom to top because it is appended to the end of l. 2.

llnʾt	1	By Ilnaʾat,
[bn] [sʿ	2	[son of] Saʿa-
dn bn ntʾt	3	dān, son of Natʾat.

lnʾt, cf. Ar naʾata "to walk, go, at a slow pace" (cf. LaAEL, p. 2751B); cf. nʾtʾl of Eut 534. – sʿdn, as in the edition; e.g., JsaT 392. – ntʾt, cf. Ar natá (i) "palam fecit, divulgavit" (cf. FrLAL, IV, p. 240A). – Jobling speaks of the surface being "severely weathered on the top left hand [sic!] corner." This is true if one looks at the photo as seen in the position represented by the facsimile. According to his photo, however, it is the lower left corner that is broken off; the facsimile shows the stone after being rotated 90° clockwise so that the bottom of the photo became the left side of the facsimile. Furthermore, Jobling's facsimile distorts the photo by representing one very short bar only in that corner. In fact, there are two of them and, according to their position, they should make a very small right angle which cannot belong to any letter on the basis of the letter height of the balance of the text; therefore, they disprove his restoration of the initial "l" at the beginning of his l. 2. The slightly aslant position of the two t's suggests that the whole text is from one hand only.

JaT 120 = JoNAT 9 (cf. pp. 263B–264A, facsimile in p. 276, and photo in p. 482A).

a = no. 9: llḍ bn ʾslm By Laḍḍ, son of ʾAslam.

This text is identical with JaT 106o (cf. above, p. 207). Jobling does not mention

this identity although he published the latter in the same year 1986. That the two texts come from the same hand is proven by the fact that the two l's of each text and the ductus of each line are identical in both cases.

b: about 6 cm to the left of the initial l of the preceding text, there are two signs, which are not mentioned in Jobling's facsimile and/or commentary. They most probably are a wasm. The hypothesis of the beginning of a text left incomplete does not seem probable because of the length and the position of the lower bar.

JaT 121 = JoNAT 10 (cf. p. 264A, facsimile in p. 277, and photo in p. 482B).

a = no. 1/1: the two left vertical lines less the bottom of l.1:

lkhlt	1	By Kahilat,
bn sr	2	son of Sarr.

khlt, as in the edition; cf. Saf, e.g., CIS 24. - sr: HaLi 114.

b = no. 2: the two extreme vertical lines and the bottom letter of the central vertical line.

l,ʾswr	1	By ʾAswar,
bn sr	2	son of Sarr. - As in the edition.- ʾswr, e.g., HaLi 427.

Jobling's commentary contains the following statements:

" The script is clear and the graphemes are well formed. The second inscription appears to have been written around the first inscription. The size and clarity of incision in the patina make this ordering of the inscriptions quite feasible."

Jobling did not see four facts which are, however, immediately apparent.

1 - If the author of text b had no intention of connecting his text with text a, he could very easily have engraved either his whole text farther to the right or the end of his text below and to the right of his first four letters.

2 - The letter w is manifestly oversized and especially when compared with r. Such a disproportion was voluntary and was aimed at filling as much as possible the space below s.

3 - The location of r below t of text a also was voluntary; it could very easily have been added directly below w.

4 - The letter b is engraved on the left of both r of text b and t of text a.

Therefore, bn sr belongs to the two texts, and khlt and ʾswr are brothers.

c : an isolated wasm. Jobling writes this:

" A hammering occurs on the right side of this rock face. This may be the beginning of an unfinished inscription. "

Of course, since his photo is upside down, the sign is found in the left section. Furthermore, it would be more accurate to speak of pecking rather than "hammering" and also of a wasm rather than "an unfinished inscription" because there is no proof, not even the slightest clue, that that sign actually is the beginning of an incomplete text. Cf. the upper half of the wasm of JaT 120b.

JaT 122 = JoNAT 11 (cf. p. 264A-B, facsimile in p. 278, and photo in p. 483A).

lʾrnlh bn nhq	1	By ʾAranlah, son of Nāhiq,
bn ʾbnl lk	2	son of ʾAbanil. He was struck on the back of the head.

Jobling's whole commentary deals with the initial name and reads as follows:

[A]" Although this inscription is quite well preserved there is some difficulty in reading the second and third graphemes of the proper name *'RFLH*. The *Rā'* could be read as *Bā* though the three instances of this grapheme in this inscription do not compare well in the proportions of the letterings The *Fā'* is poorly preserved and is obscured by a later mark. It could be read as *shfn*. Given the frequency of papponymy attested in other inscriptions in this area I am inclined to suggest that given this context the first proper name should be read as *BŠLH* which is already attested in North Arabian. "

[B]

[C]

[D]

[E]

It is really flabbergasting to see an incompetent author in action, viz. he does not see exactly what is on the stone, he sets aside what he sees to argue his point, and finally he suggests an opinion which contradicts what he had previously admitted having seen.

Ad [A] - Even before his discussion, he speaks of "the proper name ʾRFLH" (also in his translation and his word list in p. 266B) although his transliteration points two letters as doubtful, viz. "ʾR̈F̈LH." The reading of the name does not present any difficulty.

Ad [B] - The main sentence is erroneous: the second letter of the name cannot be read "b" because the letter is much smaller and more angular than any of the three b's of the text. This is most probably what Jobling intended to say in his dependent clause; but it is worded so cryptically that the real meaning of it is impossible to determine accurately.

Ad [C] - The so-called "later mark" is so undistinguishable in his facsimile that the reader is unable to find out its location. It is a short, aslant bar below the dot.

Ad [D] - His suggestion of applying "papponymy" is contradicted by his reading of "RF;" but it proves how preconceived ideas are given absolute priority.

Ad [E] - The apotheosis of Jobling's argumentation is the present phrase: his affirmation is flatly contradicted by his own p. 266B, where he writes this:" *BŠLH* possible new name. *cf. Lane* p.206-207."

Therefore, he does not even have HaAIC to refer to because the latter has no
try for "bš" (p. 106). How incredible it is! Does Jobling think that he is goi
to fool his reader as he has fooled others in the past? But, there is more to c
me. LaAEL (pp. 606-607) lists Ar bāsa; thus, Jobling understands his "ʾbšlh" as
an ʾ-qtl-lh derivation; but this formation is not attested in Tham. - For ʾrnlh,
cf. ʾrn in both Tham Ǧāʾil 176, and Liḥ JaL 146a. - nhq, as in the edition (cf.
also p. 267B); cf. Ar nahaqa "rudit asinus" (cf. FrLAL, IV, p. 345A). - ʾbnl, a
qtll (l = il) derivation; cf., e.g., ʾbn in Hu 106. - lk, in the passive voice, e.
g., in JaaT 559. - For the form of k, cf. JaT 125c.

JaT 123 = JoNAT 12 (cf. p. 264B, facsimile in p. 279, and photo in p. 483B): a vertical line to be read from top to bottom, and below the foreleg of a camel looking rightward.

wtm bn wʿl hṯṯ Wattum, son of Waʿl, has cut[this].

Same reading of the letters as in the edition. - wtm, e.g., Ramm 2. Jobling reads the name as "*W. TM*. And TM" and writes this:

"Phrases usually embedded in a larger context (*cf* such as this, which begin with *Waw*. are AM85/74B/23 & 738/15)"

The two texts referred to are his nos. 14 and 16; the author should also have referred to his no. 15. In his no. 13/2,3, Jobling erroneously cuts the so-called "context" into two independent lines, but he does not do it in his nos. 14/3,4 and 16/4; and the author fails to justify such illogical an attitude. But, his illogicalness is further exemplified by this statement which follows immediately the preceding quotation: "However the example may suggest that the *Waw* particle introduced a brief formula of authorship which could stand alone.[12]"

"12 W. Fischer (ed.), *Grundriss der Arabischen Philologie*. Wiesbaden. 1982. p 22. 85"

Here again, this is a mere statement which does not explain the reason of this inscription being so special that the initial w must be taken as a conjunction. His reason has to be compellent and is doubtlessly to be found in the publication listed in his footnote 12. In the first of the two references, W.W. Müller deals with Liḥ (not Tham) and notes on the two conjunctions f and w that "f- [ist] von w- deutlich geschieden." Such a general statement is erroneous because f sometimes has the normal meaning of w; cf., e.g., JaL 59/2 and 118/7. In the second reference, W. Fischer notes that in the early testimonies of Ar, only wa- has remained from wa-, fa- and tumma-. Therefore, Jobling did not even understand correctly the two texts he is referring to or, if he understood them, he distorted their meanings. - wʿl, e.g., Hu 569. - hṯṯ: the understood complement of the verb is the text itself and also, as suggested by Jobling, the camel above the text.

"The evidence of patina and the left of the inscription appear to be later comparative weathering suggests that the additions to this rock face," notes Jobling in p. cruder camel and anthropoid drawings to 264B.

This statement cannot be checked on the photo and does not specify the relative chronology of the last two petroglyths.

JaT 124, cf. JoNAT 13 (cf. pp. 264B-265A, facsimile in p. 280, and photo in p. 484A).

a = no. 1: below and to the left of the hindlegs of a hunted ibex which is running rightward.

lbnn By Banîn. - As in the edition; cf. JaT 105f.

b = nos. 2+3: below the belly of the same beast; l. 2 is to the right of, and parallel to, the beginning of l. 1.

lgbn 1 By Gâbin;

whkf byr mʿd 2 and Ḥâkif has tried Muʿawd.

Jobling considers l.2 as a separate text, his no. 3. Yet, he contradicts himself in his commentary, where he writes that (p. 265A)

"The situation of the second and third inscriptions suggests that it may be possible to posit that *DBN* was the author of this prayer to Dushares."

Note that a couple of letters form twice a short senestrograde line, viz. hk after a single letter, w; and yr after two letters, fb, and preceding three letters. What is the meaning of an aslant text being divided into three sections composed respectively of one, two and three letters by two groups of two letters each? - gbn, Hu 577. - hkf, cf. Saf hkft in CIS 244. HaAIC (p. 196) notes this: "Root unknown." Cf. Ar hakafa "larus fuit in opere et cunctatus fuit" (cf. FrLAL, I, p. 410B). Furthermore, HaAIC refers to the same text under "hkft" in p. 620 (an erroneous "h" instead of "ḥ") but without any cross-reference in either place. - byr, cf. Ar bāra (a) "to try, assay (someone)" (cf. LaAEL, p. 274B). - mʿd, WiAM 4.

The hunting scene is composed of a running ibex already transpierced by an arrow in the withers, two dogs running toward the beast, and behind the latter a bowman aiming at the animal. It is quite impossible to understand how Jobling could ever write that (p. 265A) "Curiously the hunter, armed with a bow is depicted facing away from the pierced caprid."

Of course, the hunter is facing the hunted beast. Who in his right mind would ever try to shoot an arrow at a beast while holding the bow behind his back? The position of the hunter's feet also is manifest. - Finally, it is difficult to figure

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out the meaning of the petroglyth above the hindlegs of the hunted ibex.

c : not recorded by Jobling; about 16 cm almost directly below the beginning of text b.

lfnn By Fanan. - Cf. fn, e.g., in Ḥâ'il 23, and Saf fnn in CIS 21.

JaT 125 = JoNAT 14 (cf. p. 265A, facsimile in p. 281, and photo in p. 484B).

Joblings states that

"Later hammerings despoil the original definition of this rock drawing and obscure the final graphemes of the first inscription."

The two parts of the previous statement are completely untrue.

a = no. 6: in the extreme left central section of the surface, two vertical lines slightly aslant to the right and parallel to each other, l.1 being on the right.

lskrn	1	By Sakrân,
bn mb^{c}l	2	son of Muba^{c}al.

skrn, as in the edition; Ḥâ'il 29. - mb^{c}l, cf., e.g., b^{c}lh of JaT 105b. - Jobling reads the second name as "mb^{c}ln" and states that "In the sixth inscription the last two graphemes are difficult to read."

The letter l is very clear, and his "n" is nowhere to be seen.

b = no. 5: slightly below, to the right of, and parallel to, the lower half of the preceding text.

lkdlh By Kadluh. - Cf. Ar kadala "repulsit" (cf. FrLAL, IV, p. 40B).

c = no. 4: two vertical lines aslant to the left, to the right of, and below the preceding text; and to the left of the camel.

^{c}lnyt bn kmn	1	^{c}Alnîyat, son of Kamîn,
ts	2	has gone away.

Jobling writes this on the two initial letters: "In the fourth inscription there appears to be a ligature connecting *Waw* and *Lam*."

There is a ligature; b u t both ends of the long bar are not curved; were it the case, the whole sign would be the letter d. But, the right end of the bar only is curved. Furthermore, Jobling has taken as the right section of his "w" an extremely narrow feature of the stone, and this feature is black in the photo while the letter itself is whitish. Cf. Ar ^{c}alânîyat "openness, publicity; one's outward man" (cf. LaAEL, p. 2142B); cf. also ^{c}ln, e.g., in Eut 47a. - kmn, Hu 173. - ts = tss of Hu 412 a/1.

d = no. 3: below the belly of the camel.

l^{c}kbr By ^{c}Ikbir. - As in the edition; cf. Saf y^{c}kbr in WH 1749.

e = no. 1: above and to the right of the head of the camel.

lwtr bn ^{c}m By Watar, son of ^{c}Amm.

As in the edition. - wtr, e.g., Rho 8. - ^{c}m, e.g., JaT 7 a/1.

f = no. 2: below and to the right of the preceding text.

lbn ḫrb By Bayn. He has plundered [here].

Same reading of the letters as in the edition. - lbn, also in Hu 424. - ḫrb, cf. Saf, e.g., BM 12098. WiTh 64 (cf. WiReAR, pp. 110-111, facsimile in p. 210, and photo in p. 220, lower left corner) may be read as follows: bh yr^{3} whl rfm

Has become fatigued Yarav^{3} and he has camped at Rawfum.

bh, Hu 512. - yr^{3}, cf. r^{3}t of JsaT 504. - hl, e.g., Ramm 131/2. - rfm, cf. Saf, e.g., rf of Wi 17.

JaT 126 = JoNAT 15 (cf. p. 265A-B, facsimile in p. 282, and photo in p. 485A).

a = no. 1: in the left section of the panel.

l^{c}lyn bn qnt	1	By ^{c}Alyân, son of Qannat,
bn n^{c}my bn rbqt bn ^{c}rk	2	son of Na^{c}amay, son of Rabqat, son of ^{c}Arik,
bn ^{c}bd	3	son of ^{c}Abd.

Same reading as in the edition. - ^{c}lyn, cf. ^{c}lyt in JsaT 295. - qnt, e.g., Hu 593. - n^{c}my, cf., e.g., n^{c}ml of Hu 149+151/1. - rbqt, cf. Saf srbq in CIS 391. - ^{c}rk, cf. Ar ^{c}arik "stong, vehement" (cf. LaAEL, p. 2024A).

b = no. 2: in the lower right section of the panel, below and to the right of both l. 3 of the preceding text and the beginning of text c.

lsmdt bn ddlh bn hn^{3}lh By Samdat, son of Dawdlah, son of Hani^{3}lah.

smdt, as in the edition; JaT 101. - ddlh, JaT 105d. - hn^{3}lh, as in the edition; e.g., HaLi 438.

c = no. 3: to the left of a/3, and below the left half of a/2 and the end of a/1.

srr^{c}t dlby ll rk Sarra^{c}at, he of Labîy, has hired Rakk for the night.

srr^{c}t: first example of the qttlt (reduplication) in Tham; cf. Ar saru^{c}a "to be quick, expectitious, hasty" (cf. LaAEL, p. 1348B). - The qttln (reduplication) derivation exists in Tham; cf., e.g., rttgn of JaT 34 a/3. The qttlt (reduplication) formation exists also in Saf; cf. hnn^{3}t of Wh 2975. This name is another example of F.V. Winnett's fancy workmanship. WiHaI's transliteration (p. 427) has "hnn^{3}t" which is confirmed by the facsimile (p. 712). Then, Winnett's translation gives "Han()Ĭat" (p. 427), and his commentary informs the reader of this: "[illegible]. not in HIn." This remark is untrue: HaAIC mentiones hnn^{3}t of this text at the top of p. 628; WiHaI's p. 621A lists "hnn^{3}t 2975. Emend to hn^{3}t." This affirmative

statement, however, becomes doubtful in p. 620B because of the entry "hn²t ... s'n 2975?" This is the way Winnett eliminates an epigraphical fact because of his erroneous preoccupation of reading the inscriptions with the purpose of finding names already attested somewhere else. Unfortunately, the edition of the text (p. 621) does not mention the existence of any photo. Finally, note that hnn², a qttl (reduplication) derivation, also exists in Saf, e.g., of CIS 3292. - The last four letters, which Jobling reads as "KLLH," are commented upon by him as follows:

> " While the letters *LL* are reasonably clear the lettering of the initial *K* and final *H* are disputable."

Note "are" instead of "is." His reading is inaccurate: the presence of two different forms of l in the same text is already attested in this collection by JaT 112b, 115b, d, etc. - lby, e.g., JaT 118b. - ll, cf. Ar lâyala, 3rd form "protectionem alicuius pro una nocte imploravit c.a.p.; pactus fuit cum aliquo pro una nocte" (cf. FrLAL, IV, p. 142B). - rk, cf. Saf, e.g., Ox 228; cf. also the verb rk in Tham JsaT 339.

d: eliminated by Jobling; below the end of the preceding text and to the left of the end of text b.

lbnn By Banîn. - E.g., JaT 124a.

I - Conclusions.

The only possible way of summarizing the preceding pages devoted to some of Jobling's publications of Tham inscriptions is to formulate the basic features of his work. These features, exemplified all through his papers are complete incompetence, superficiality, illogicism and extravaganza in his editing work as well as in his facsimiles. It is against such a background that the following quotation must be evaluated. JoAMA, III, p. 245A, publishes the following statement:

> " a catalogue of names so far recorded is in preparation along with a discussion of the etymological and historical significance of this data within the wider context of comparative Semitic linguistics"

(cf. also, JoNAT, p. 261A; cf. above, p. 218).

What a dreadful perspective to contemplate for anyone who does not, and should not, forget that Jobling himself kept repeating that his editions of texts "are only preliminary studies" (cf. above, e.g., p. 217). Furthermore, JoNAT (p.261A) informs the reader that

> " Microfiche and interactive videodisc editions of these records are in preparation."

In fact, the reader must brace himself to face "a catalogue of names so far recorded" and especially in the light of the author himself giving the clear proof of his inability of building up a decent word list of a small collection of 38 (according to him) short texts, as exposed above, pp. 229-230. It is going to be another HsAIC (cf. my review in JaMAR, II, pp. 93-150), based on an incompetent study of the inscriptions. Finally, what is most disturbing is that Jobling seems to gorge himself with high-sounding expressions which he does not seem to understand. JoNAT publishes the following affirmation (p. 261A): "

> extensive research has been undertaken into the history and current state of the art of North Arabian epigraphy and paleography."

What the 90 preceding pages make most obvious is that Jobling does not understand what he writes because his expression "extensive research" is not only an empty babbling but is just the opposite of what his papers offer.

It is not at all surprising to see the facility with which Knauf's and Voigt's concoctions are picked up by the authors working in Jordan. It is, however, highly significative and immediately understandable; there is no need to dwell on it.

On the one hand, Jobling's treatment of the few Tham texts he selected for his papers unfortunately proves how embryonic is his knowledge of a field in which he so stubbornly keeps wandering about. His errors are so numerous and so obvious; his mistreatment of photos and his disregard for the precious remains shown on those plates are so manifest; his argumenting method is so askew that his papers impose an unwelcome surplus of work. Consequently, his work was already unworthy of being printed.

On the other hand, his papers shows a progressive widening of his reading material, and this, in turn, is reflected in a progressive utilization of fancy words and expressions. In the meantime, however, his knowledge and understanding of Tham remains stagnant at the embryonic level.

The preceding remarks lead to two important deductions. The first deduction is the following question: How can the reader expect Jobling not to indulge in the same dilettantism and superficiality when dealing with archaeology?, viz. could the reader believe in careful studies, precise presentations and valid argumentations in archaeological matters when he sees the opposite all through the epigraphical sections? The second deduction is that the final publication of the epigraphic material could not possibly be reliable when the facts of the present pages show even less objectivity and less knowledge than W.A.Clark in his thesis in Saf (cf. my review in JaMAR, XIII, pp. 2-113) and/or WiHaI (cf.

my review in Orientalia, 48 [1979], pp. 472-528). History will, I am afraid, repeat itself. When G.L. Harding visited me in early 1963 in connection with his project on an index and concordance of pre-Islamic names, I did my best, I went as far as possible - but it was of no avail - to make him understand that SA was not his field and even that the previous publications on NA could not be trusted but had to be revised completely. Alas he could not see anything but his project. The shortcomings of his HaAIC are so obvious and so numerous that it is a wholly unreliable publication.

Summarily, the whole situation is appropriately explained and illustrated by the basic confusion of the author who in the same year 1984 boasts not only that he has "expertise" in those inscriptions but also that "I still have a long way to go in North Arabian studies" (cf. above, pp. 160-162).

IX - The inscribed antiquities from the Alia Airport Cemetery, Amman, Jordan, viz. the Thamudic slab JaT 131 and the Ḥaḍramitic seal Ja 3204.

In early June 1978, some construction workers at Queen Alia International Airport stumbled on ancient graves, and the results of the excavated area are released by M.M. Ibrahim and R.L. Gordon in A Cemetery at Queen Alia International Airport (Yarmuk University Publications, Institute of Archaeology and Anthropology, Vol. 1), Wiesbaden, 1987, with appendices by four collaborators. "The site lies some 25 km. south of Amman, 22 km. east of Madaba" (p. 14), and the excavators identified 173 tombs (p. 91). The excavation of tomb 1 began on "12 June" (p. 71) and that of tomb 130 on "1 July" (p. 89); no date is given for tombs 131-173. Among the findings unearthed during the excavation campaign are two inscribed objects, viz. a Thamudic slab and an elliptical Ḥaḍramitic seal. These two antiquities are published in appendices "A" (pp. 43-45) by Wolfgang Röllig and "F" (pp. 46-47) by Ernst A. Knauf, respectively. These two studies, however, are based on incompetence and dreamy premisses, respectively; they also are heedless of both published material and good common sense.

A - Introduction.

The following notes listed under the preceding label were collected during a preliminary reading of the work which revealed not only the poor organization of the material at hand but also the absence of any systematic and thorough editing of the various sections of the book.

1 - The "Museum Listing" (pp. 57-70, nos. 13496-13588) does not follow the numerical order of the tombs which are described in the "Cemetery Data" (pp. 71-92), and the museum sigla are never utilized either in the text or the plates. The reader has to devote a lot of time finding out if any particular item mentioned in the second list is dealt with in the text and/or is reproduced in the plates. Furthermore, as both lists deal with the same material, they necessarily contain a great number of duplications, and the details of the two lists are too often different. It would have been advantageous

to avoid such unnecessary overlappings and discrepancies by centering the whol presentation on a single list and by complementing it, when necessary, with t information of the other. Not only all duplications, which are quite superfluou would have been eliminated, but also the possibilities of discrepancy would have been very greatly reduced. Moreover, a careful editing of the manuscript would have straightened out many anomalies which suggest that the various sections of the book were composed independently with very few additions here and there (but nothing systematic); a good editing would also have moulded all sections into a well-organized unit; viz. the two appendices "A" and "B" almost stand by themselves as two islands with no cross-references with the text.

As explained in p. 71, the three sections of each excavation siglum give the numbers of "Grave/Object/Museum," respectively. However, the third element is not the museum siglum, but only its last two digits. The following exceptions, however, are noted, viz.

"13524a = 91/37/24" (p. 60); "13524b = 97/31/24" (p. 60);
"13530 = 60/37/29" (p. 61); "13535 = no siglum" (p. 62);
"13537 = 10/45/36" (p. 62); and "13572 = 85/92/73" (p. 68).

2 - Some items now in the museum are neither utilized in the text nor shown in the plates; viz.

"13500b = 75/5/00b" (p. 59) = "75/05/00," first mention (p. 81);
"13516 = 75/21/16" (pp. 59 and 81);
"13522a = 75/29/22a Leather Fragments" (p. 60) = "75/29/22 Leather cap?"(p.81);
"13525 = 97/32/25 Copper Ring" (p. 61) = "97/32/25 Copper earring" (p. 84);
"13540 = 14/49/40 Approximately 39 Glass Beads" (p. 63; but only 35 are listed) = "14/49/40 39 Glass beads" (p. 73); and
"13559 = 52/72/59 2 Bronze Rings" (pp. 65-66) = "52/72/59 Bronze finger rings" (p. 77).

3 - Some items now in the museum are not shown in any plate, but are mentioned in the text; viz.

"13502 = 75/7/02" (p. 58); cf. pp. 29-30;
"13512 = 75/17/12" (p. 59); cf. p. 24;
"13517 = 75/23/17" (p. 59); cf. p. 25;
"13524b = 97/31/24" (p. 80); cf. p. 21;
"13534 = 5/42/34" (p. 62); cf. p. 22;
"13551 = 39/63/51" (p. 64); cf. p. 24;
"13588 = 97/124/88" (p. 70); cf. p. 30.

4 - Some items not in the museum are mentioned in the text; viz.

"79/29/- one pierced bronze disc [with] a pin through the centre" (p. 21) = "75/29/21 Bronze button cover, pin through center" (p. 81). Both spellings are used together in p. 67 in 13565; for "centre" and "center," cf., respectively, p. 15 (twice), 20 and pp. 38, 57 in 13499, 90 in no. 137.

"74/-/- fragments of bronze pins" (p. 21); "four copper wires" (p. 24); "four rings with thin bezel plates" (pp. 25-26); and "bone kohl tube" (pp. 28-29) All those items and others are listed under the number 74 (p. 80) without any number.

"67/-/- Simple copper loop bracelet" (p. 24). Among the items from grave 67(p. 79), there is only one mention of one "Bronze wire bracelet (in photo)," but there is no photo of that grave in the plates.

"36/60/- Simple copper loop bracelet" (p. 24). Among the items from grave 36 (p. 75), there is one "Bronze bracelet (reused spatula handle) 36/-60," and this description is given in p. 24 (cf. above) and in p.28 where it is specified that the spatula was broken before being reused as a bracelet.

5 - There is apparently no order in the photos of the tombs, scattered, as they are, in four different places; viz. pl. 3-9, 25A, 36 and 43A-B; and they are not arranged numerically. Furthermore, there is no reason for not printing the two photos of tombs 14 and 62 on the same page where their study would have been much easier rather than in pl. 5B, 7A and 3B, 4B, respectively. The tomb numbers "199" (p. 25), "75bis" (p. 29) and "103" (p. 29) should be amended to 119, 75 and 103A, respectively. Finally, the reference "105 x 3" (p.30) remains unexplained; it means "105, 3" "spherical glass vessels" listed in p. 86, but without any siglum.

6 - The spelling "millifiore" instead of "millefiore" is constant; cf. pp. 9, 26 and 27; in 13535 (p. 62) + no. 5(p. 72); in 13548, four times) (p. 64); in 13553 (p. 65); in 13560a (p. 66) + no. 52 (p. 77); in 13560c(p. 66); in 13560d (p. 66); in no. 99 (p. 84), in no. 4 (p. 92) and pl. 32Bb and 34C.

7 - The first two sections of the excavation siglum.

When each of the first two sections is a number 0 - 9, the number is never preceded by "0" in the "Museum Listing;" viz. p. 57, "0/1/96," "3/2/97," 3/3/-98, "75/4/99," "75/5/00a," etc.

The "Cemetery Data," however, adopts two different methods. In p. 72, there is no "0" in "3/2/97" and "3/3/98;" but the "0" appears

in p. 80: "75/06/01" (cf. "75/6/01" of p. 58); "75/07-08/02-03" (cf. "75/7/02" and "75/8/03" of p. 58), and "75/09/04" (cf. "75/9/04" of p. 58);

in p. 81: "75/05/00" (cf. "75/5/00a" of p. 57, and "75/5/00b" of p. 58).

The plates also adopt three different ways in the question, viz.

either both systems simultaneously:

"0/01/96 (pl. 35, 36), but "00/01/96" (pl. 53);

"3/2/97" (pl. 6A, 20A, 41), but "03/02/97" (pl. 19);

"3/3/98" (p. 6A, 23A), but "03/03/98" (pl. 24); and

"75/4/99" (pl. 51C), but "75/04/99" (pl. 37A);

or one system only, and that system may be

the first: "75/5/00" (pl. 28C); "76/6/01" (pl. 21A and 51A); and "75/8/03" (pl. 21B), or

the second: "75/09/04" (pl. 22); and "05/43/35" (pl. 32Bb).

7 - The last example of 6 (above) on the list of "Cemetery Data" is one example illustrating the fact that the "Cemetery Data" sometimes gives a single number to several objects which are presented separately in the "Museum Listing." Other examples are as follows:

"Museum Listing"	"Cemetery Data"
"13500a = 75/5/00a" (p. 57) } "13500b = 75/5/00b" (p. 58) }	"75/05/00" (p. 81, twice)
"13514a = 75/19/14a" (p. 80) } "13514b = 75/19/14b" (p. 80) }	"75/19/14" (p. 80).
"13522a = 75/29/22a" (p. 60)	"75/29/22" (p. 81)
"13522b = 75/29/22b" (p. 60)	"75/29/22 (p. 81, twice).
"13524a = 97/31/24" (p. 60) } "13524b = 97/31/24" (p. 60) } Contrary to all examples, there is no "a" and "b" after the second siglum.	"97/31/24" (p. 84).
"13560a = 52/73/60a" (p. 66)	"52/73/60" (p. 77).
"13560b = 65/76/60b 22 beads:" followed by 34 items (p. 66).	"65/76/60" (p. 78): un unspecified amount of "beads."
"13560c = 66/78/60c" (p. 66):"19 beads"	"66/78/60" (p. 78): "19 beads."
"13560d = 67/79/60d" 4 beads followed by 5 items, including "2 glass - 1 millifiore (sphere,marbled black and red)" and "1 barrel" (p. 66).	"67/79/60" (p. 79 4 lase beads, including "2 marbled (1 barrel)."
"13570a = 73/86/70a 3 bronze rings" (p. 67).	"73/86/70 (p. 80) 2 rings + "136/116/70" (p. 90): 1 ring.
"13570b = 76/87/70b Copper/Bronze Armlets" (p. 67)	"76/87/70 2 bronze twisted wire armlets with cape" (p. 81).
"13570c = 76/88/70c bronze knob" (p. 68).	"76/88/70 broner knob(handle)"(p. 81).
"13579a = 118/105/79a Bone Disc and Plaque and Studs" (p. 69)	"118/105/79 2 Bone studs ('chess pawns') and disc" (p. 88).
"13579b ,c = 118/105/79 Bone plaque. Third tack. Fragment of similar plaque. Two bone studs" (p. 69).	"118/25+105/79+19 3 bone plaques"(p.88). The preceding siglum apparently comprise 118/25/19 (p. 60) and 118/105/79!!!
"13582a = 117/119/82a 2 bronze rings ... corroded together"(p.69).	"117/119/82 2 bronze heavy hinged rings" (p. 88).
"13582b = 119/110/82b rings" (p. 69).	"119/110/82 finger rings" (p. 88).
"13582c = 136/115/82c" (p. 69)	"136/115/82" (p. 90).

8 - Other examples of discrepancy of contents between the two lists.

"13525 = 97/32/25 Copper wire ring with straight overlapping ends (p. 61).	"97/32/25 Copper earring, fine wire" (p. 84).
"13538 = 13/47/38 Copper bracelet" (p. 62; cf. also p. 24).	"13/47/38 Bronze Bracelet" (p. 73).
"13543 = 14/52/43 Copper wire Bracelet /Anklet" (p. 63); cf. also "copper bracelet" (p. 24).	"14/52/43 Bronze wire bracelet" (p. 73).
"13557 = 42/69/67 Bronze spatula" (p.	"AE spatula 42/69/57 Beads 42/69/57" (p. 76).
"13574 = 100/96/74 probably for finger" (p. 68).	"100/96/74 finger ring" (p. 85).

9 - The plates and the text.

The plates and their utilization in the text show an unusually high number of abnormalities and/or discrepancies. I have filled 18 long handwritten pages with remarks on the subject; of course, it would be tedious to reproduce them.Here are a few examples.

Pl. 1A: the reference to this plate instead of to pl. 6B on p. 17 is unjustifiable.

Pl. 1B: it is very difficult to distinguish only "four distinct layers" (p. 15, second mention) in the photo.

Pl. [illegible] is referred to thrice in p. 17 in the following expressions,

"(Graves 4, 14, 16, 80, 89, 92, 93; Plates VII and XLV) ;"

"(Graves 4, 14, 23, 117?, 133, Plate VII.1) " and

"(Graves 82, 89, 93; Plate VII.2)"

Pl. 7 A and B concern graves 14 and 89, respectively; and pl. 45 contains the plans of graves 89, 111 and 114. Thus, the plates referred to do not even concern the first number of each series; and no photo shows any of the graves 4, 16, 23, 80, 82, 92, 93, 117 and 133! The authors certainly have a very unusual way of making the references.

Pl. 12B: "Armlets and Knob from Grave 76: 76/88/70b 76/87/70c." Obviously, the first siglum refers to the two armlets on the right, and the second to the knob on the left.

1 - "76/88/70b"

= "76/88/70," second mention: "bronze knob (handle)" (p. 81; cf. also p. 31)

= "76/88/70c" = 13570c which speaks of a "Bronze Knob."

The object was used as a pendant.

2 - "76/87/70c Armlets." The authors did not realize (p. 23) that the sketch of pl. 49C represents the lower armlet.

= "76/87" of pl. 49C;

= "76/87/70" (also p. 23), viz. one of the two armlets listed in the first information (p. 81)

= "76/87/70b," one of the "Armlets" under 13570b in p. 67, where "XII" should be corrected to 12B, and where the reference to pl. 49C should be added.

The armlets are said to be of "bronze" (p. 81);
"copper" (p. 23) and
"Copper/Bronze" (p. 67 and pl. 49C).

Pl. 14A: "85/95/72"

= "85/92/72" (pp. 25 and 82) and

= "85/92/73" (p. 68), but this is an error because of the last digit of the museum siglum, 13572.

The items are "hoops" (p. 25), but
"Bracelets/Anklets" (pp. 68 and 82); and to accommodate both,
"hoop anklets" (pl. 14A).

Pl. 17B: "66/105//." = "66/105/-" (p. 26) = "66/105" [illegible]

ze mirror. Most remarkable is that the item is complete except for a small central, trapezoid hole in the sketch of pl. 49D, but pl. 17B shows a large section missing in the right center to the left of the aforesaid hole. Here, however, as it was done by J. Pirenne in her facsimile of the bucrania of CIE 695 (cf. above, p. 26) and in F. Zayadine's paper in ADAJ, 24 (1980), p. 108 in the case of Saf J 13157 (cf. JaMAR, XIII, p. 139), the facsimile was made from the print of the black-and-white photo on some transparent paper and retraced on the reverse of the paper, and it is this retracing which is used in pl. 49D. But, the discrepancy between the photo and the sketch is not mentioned anywhere. Finally, this item did not find its way in the "Museum Listing."

Pl. 26 and 27. - The caption of pl. 26 says that the photo shows "Terracotta Figurine 60/37/29 in situ" (cf. also p. 8). I do not see the object. That figurine is represented in pl. 27 also as "60/37/29," which is irregularly given the museum siglum "13530" (p. 61). But, the only "Terracotta Figurine" listed in p. 77, under no. 60, is "60/37/30;" cf. also p. 30, which refers to pl. 26 and 27.

Pl. 32B is composed of four photos, a, b, c and d, with five captions; and there is no information indicating to which photo each caption refers.

a : upper left: 34/54/45" (first caption) : 25 items as in 34/54/45, second mention: "23 glass, 1 frit."

b : upper right: "05/43/35 Millifiore Beads" (second caption): 3 items;

= "5/43/35" (p. 72): "Glass beads millifiore x 3;"

= 13535 (p. 62) which repeats the description of p. 72; the siglum "05/43/35" should be added to the entry; and "XXXII.2" be corrected to 32Bb.

c : bottom left: "85/91/. White Glass Sandwich Beads" (third caption); 9 items;

= "85/91" (p. 82): "9 glass beads" but this introduces "2... and two ...sandwich ... 3... and 2... 3," thus, 12. Not entered in the "Museum Listing."

d : bottom right: "33/58/48" (fifth caption); p. 75 also has "polychrome earrings" as in pl. 32B, where "earring" is in the singular;

= 13548 (p. 64): "Polychrome Earring(s)," where "XXXII.2" should be corrected to 32Bd.

The fourth caption "Carnelian and Glass ...33/57/47" = 13547 (p. 64), which

does not apply to any of the four photos, although it refers to "Pl. XXXXI.2" pl. 32B. The information of p. 9 describes the distribution of the original pl. 32B which was composed of five photos with a blank lower right, viz.

a = 34/54/45; b = "5/43/35;"
c = 85/91/-; d = 33/57/47;
e = 33/58/48.

The contents of the original of pl. 37B

The contents of the printed pl. 37B

In the printed plate, "d" (= 33/57/47) was dropped and replaced by c; and "e" (= 33/58/48) remained in place but automatically became the new d.

Pl. 37B = 20/53/44; four photos, a-d, and 2 sketches in pl. 54Aa and b.

"Proto-Arabic Seal" (pl. 37A),
"Proto-Arabic Seal of Green stone" (pl. 54A);
"Green stone (serpentine?) seal" (p. 74);
"Green Stone inscribed Seal" in 13544 (p. 63);
"the seal of dark green stone" (p. 22), which "could be one of the dark green copper minerals, such as malachite ... [but] it resembles serpentine" (p. 22). Therefore, the accurate identification of the stone is wanting. However, no information is given to the effect that the seal was submitted to a geological laboratory. Furthermore, the reader has to go to p.26 to find out that this seal was "set into [one of] these" "iron rings," an information which cannot be guessed at from the photos of pl. 37B. And Knauf's so-called study of the seal (pp. 46-47; cf. below) does not even refer to p. 22 and especially to p. 26!!! In pp. 22, 46, and 13544 (p. 63), correct "LIV" to 54A.

Pl. 38 : "136/117/83;"
= "136/117/84" (pp. 23, 26 and 90); and
= "136/118/84" = 13584 (p. 73).

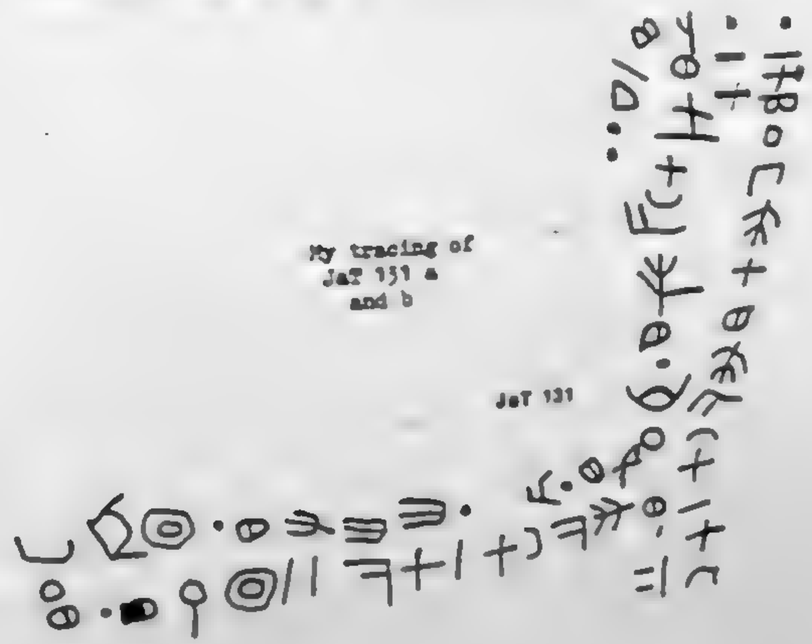
This is another item known by three different sigla!

B - The inscribed Tham slab JaT 131 (photos in pl. 35 and 36).

1 - Introduction.

a - A simple glance at the photos shows that the inscribed surface itself was not damaged after the engraving of the letters. The introduction (p. 34), where the reader finds the physical description of the slab, states that "It is indeed possible that a few chips have been lost from one corner of the stone (bottom left in Plate XXXVI, upper right in Appendix A, Figure 12, as convention dictates)."

Facsimile of RōTI A-C by "Ms. Jennifer Cowherd" (pp. 10 and 14)



My tracing of JaT 131 a and b

JaT 131

The tiny section of the stone involved is located immediately below the almost vertical right line of text a. But, it is not whiter than many other places, as its condition is unimportant because the text remained untouched. Furthermore, the expression "bottom left in Plate XXXVI" is correct if one looks at the plate without rotating the book, but the reader must rotate the book 90° to read the caption properly. In fact, however, the caption is printed below the bottom of an upside-down photo. Moreover, the expression "upper right in Appendix A, Figure 12, as convention dictates" does not make any sense. As a matter of fact, "Fig. 1" (p. 16), which is a sketchy "Plan of Maintenance Hangar Area showing Cemetery and Hills" is the only "Fig." in the whole volume; cf. also p. 11, where the heading "Figures" (in the plural) introduces a single entry! Note also that fig. 1 is the only place giving the orthograph "'ALIA" with the unusual transliteration of ʿ as "'" (cf. علياء of the Ar title page); anywhere else (pp. 1, 3, 4, 23, etc) it is always "Alia." The mention of "Figure 12" is one example of the superficial reshuffle of the various sections of the volume because the original "Figure 12" became pl. 53, which is reproduced above, p. 259A; and "Appendix A" of the caption refers to the section of the book "Appendix A" of pp. 43–45. Finally, the phrase "as convention dictates" is meaningless and completely out of place.

b – The siglum of the slab is "0/01/96" (pl. 35 and 36), but "00/01/96 (pl. 53); no siglum is given on p. 34 or in Röllig's study. Nowhere is there any reference to the "Museum Listing," where the siglum is "13496 = 0/1/96" (p. 57); cf. already above, p. 254. This is another item known by three different sigla (cf. already above, p. 258).

c – The measurements of the slab as given in the "Museum Listing" (p. 57) contain one gross error; the text reads as follows: "Block: L. 30.2; H. 20; Th. 0.5
L. 50; H. 20; Th. 20 Second Smooth Surface:
Main Inscription Surface: L. 16; H. 15.5"

It is impossible even at first sight that the thickness of the slab be "20" cm and that of the inscribed section "0.5" cm only; this section would long time ago have been smashed to pieces. In fact, the aforesaid quotation is a good example of the carelessness with which the "Museum Listing" was written. The thickness of the inscribed section obviously is the same of that of the block itself because it is the latter's main section. The measure of "0.5" cm is, as stated in pl. 35, the difference in level between the two horizontal planes of the surface of the stone, the left plane being "0.5" cm lower than the other. A similar difference in level also exists in both the upper right corner and left side of the stone. Thus, the text of p. 57 must be corrected in two places as follows: "20" must replace "0.5" in l. 4; and "Th. 19.5" must be added at the end of l. 6. Furthermore, the facsimile of pl. 53 also is inaccurate. The scale of the facsimile, which represents the area of texts A and B, shows the measurements to be 31.3 x 19.4 cm. Since the length of the other section of the slab is about half of that of the first, the total length of the slab is about 48.95 cm.

d – The Tham slab is called "stone" (pp. 34, 43, and pl. 35, 36 and 53) or "block" (p. 57) or "slab" (p. 43); parallelly, the "covering stones" (p. 17, twice) become "Cover Slabs" on pl. 4 and 5, where most of the covers are small rocks with a few rare slabs here and there.

2 – Wolgang Röllig's "Appendix A: The Thamudic Inscriptions" (pp. 43–45, and facsimile in pl. 53).

The editor's study is childishly written because he felt obliged to write all(?) thought that sprang in his mind even when the remark turns out to be a manifest error. All that he actually achieved is to prove his incompetence and the superficiality of his work.

a – There is no reference to any of the corresponding plates, viz. pl. 35 (the whole slab), 36 (an enlargement of the texts a and b), and 53 (the facsimile of the three texts), or to any page where the stone is described or listed, viz. Röllig apparently wrote his paper in the isolation of his ivory tower.

b – Röllig's introduction (p. 43) deals with several questions.

(1) His first statement is to the effect that "It is no more than a guess that the stone had once been a covering slab in one of the graves."

(a) The translation of his text B mentions "all (men of) lḡyʿ and ʿbdḡ and ḥṭṭ" plus "blsn (?)" (p. 44), to whom must be added the two men of his text A (cf. below). But, the great majority of the tombs only contain a single body each (p. 15) while seven others have two bodies each (p. 17), and graves nos. 75 and 111 three each (p. 17; cf. also pl. 45); and the measurements of the tombs are about 2/2.50 x 0.45/0.75 x 0.50/1m (p. 15). Therefore, it is ridiculous to even think that all the men mentioned in Röllig's texts A and B be buried in a single tomb. There should have been many tombs to accommodate all those bodies and, consequently, why should the slab be on any of them? And which one would it be? And why would it be on one in

particular rather than on the next one? Incidentally, the nine tombs with more than one body each are all located, according to pl. 44, in the central third of the two lateral sections of the cemetery, viz. nos. 4, 14, 16 on the left side, and the others on the right.

(b) Röllig erroneously presupposes, but without any proof, that all the men listed in his texts A and B, with the exception, I presume, of the author himself of his text A, were dead. But, the Thaa slab was a commemorative stone, not a tombstone.

(c) The location and the level of the place where the slab was originally lying remain unknown according to the following statements, viz.:

> " During leveling of the maintenance hangar area, a number of tombs and an inscription had been noticed. The head of the police station saved the Thamudic inscription which turned out to be one of the key finds for the interpretation of the site
>
> The tombs were covered by relatively level soil 1.50-2.00 m. thick " (p. 13).

> " The stone was unfortunately not found in situ, so its relationship to the cemetery remains unclear.
>
> It was noticed by the supervisor of the levelling operations when it was tipped up by a bulldozer " (p. 34).

The relative sentence of the second statement of p. 13 is ridiculous and contradicted by the first affirmation of p. 34. When facing graves with human skeletons, nobody needs an inscription to understand the evidence, viz. that the site was a graveyard. Furthermore, there is nothing in both the reproduced quotations or the balance of the book hinting that the slab was actually found within the cemetery limits (p. 13) rather than outside, or at the tombs level rather than within the "1.50 - 2.00 m." (p. 13) of the upper soil. On the contrary, three facts suggest that the slab was originally found lying in the upper layer of the undisturbed soil somewhere close to, but west of, the general area of the ancient cemetery.

((1)) The stone was picked up unnoticed in a scoop of soil at the very beginning of the leveling operation. Had the worker noticed it, he would have had no difficulty in spotting the place; yet, nobody remembers. It may thus be assumed that nobody even knows which bulldozer picked it up. Indeed, a stone of 50 x 20 x 20 cm (p. 57) can easily be scooped by a bulldozer without being noticed and especially when the driver does neither expect nor suspect the presence of an inscribed stone and, therefore, is not attentive to the kind of rock his machine may pick up. Thus, the expression "when it was tipped up" to characterize it the moment meant to convey the idea that the supervisor became aware of the stone at the very moment it was lifted by one machine. Furthermore, no remark of the text specifies that the supervisor was accompanying that very bulldozer.

((2)) Supervisors do not stay that close to a working bulldozer to clearly see what it picks up unless he is aware of or suspects something unusual or special, but such certainly was not the case. Thus, the slab was noticed at the end of the run of the bulldozers, but not at the run of the bulldozer that brought it in otherwise the supervisor would have known exactly the run of that particular driver ... and would therefore have known exactly the level of his run. Instead of all those important details, the reader has to be satisfied with the first statement of p. 13 reproduced above. The preceding interpretation of the published data seems to be the message of Röllig's note (p. 43), which reads as follows:

> " The stone was discovered after it was moved out of its original position by bulldozing. " (my underlining).

It would also have been of great help to know where the construction workers started their leveling job, but no detail on the question is to be found in p. 13, and the information reproduced above speaks of "the maintenance hangar area," while the sketchy plan of fig. 1 mentions only the "new 'Alia maintenance building." No detail is given on their relative locations. And to render the question even more confusing, fig. 1 (p. 16) sketch of a rectangle (5.3 x 5 cm = 152 x 142 m, according to the scale) represents the elliptical area shown in pl. 44 the caption of which is "Plan of Cemetery showing Approximate Positions of Graves;" and the two axes of the elliptical plan are 14.1 x 12.2 cm = 89 x 79 m, respectively. Thus, not only the shapes of the area are dissimilar, but the measurements of fig. 1 are so different from those of pl. 44 that it is hard to believe that they both deal with the same area. Therefore, it is a gross error of methodology not only to consider as "one of the key finds for the interpretation of the site" (cf. above, p. 262; my underlining), but also, as Röllig does, that it could have "once been a covering slab in one of the graves".

((3)) The reader is not left without any clue as to the original location of the Thaa slab. It was discovered before any tomb was; and tombs 1-12 are located in the center of the western edge of the cemetery area; and, finally, the workers, at least in the beginning, pushed eastward with a short westward thrust with tombs 15-17; it seems, therefore, logical to as-

Location of tombs 1-21 in pl. 44

sume that the Tham slab was originally lying in the upper 30 cm soil crust west of tomb 7 and therefore, west of the actual border of the ancient cemetery.

(d) The relationship of the Tham slab and the cemetery according to the authors.

According to the preceding paragraph, both the level and the location where the slab was originally lying indicate that there could not be any physical relation between the slab and the cemetery. However, as far as the authors are concerned, they know nothing about the original level and location of the Tham slab so much so that they state (p. 34; cf. above, p. 262) that "the stone was unfortunately not found in situ, so its relationship to the cemetery remains unclear." Their inference, however, indicates that they still wish to by hook or by crook find some physical relation between the slab and the cemetery, as already stated in the relative sentence of their second statement reproduced above, p. 262, viz. "which turned out to be one of the key finds for the interpretation of the site." In spite of the authors' repeated avowal of lack of knowledge of the essential elements of the question, the last two pages, pp. 40-41, of their "Conclusions" (p. 35) = "Conclusion" (p. 5) offer an assortment of most surprising affirmations.

((1)) It all starts with two bewildering statements according to which the Tham slab "is roughly contemporary with the cemetery. It was found in the cemetery area." The first affirmation is an unfounded wishful thinking which is contradicted by everything they wrote before. Furthermore, it really is a fantasy to prove the contemporaneity of the objects by the fact that they were found in the same area. E.A. Knauf already used that specious argumenting method in the question of the hammer of J. Qarnat (cf. above, pp. 131-132).

((2)) The authors apparently do not understand the value of the words they use because their next statement cuts the ground from under their own feet. They write that "its precise original position in relation to the graves will never be known." (immediately after the preceding quotation). Where, then, is the basis of either affirmation reproduced immediately above? It should also be added that, as said before, that original position could not be known because it was apparently impossible to know which of the "earthmover" (p. 24) brought it in and from where.

((3)) The following statement is to the effect that

> "we have shown that no burial in this cemetery can be considered intrusive, in the sense of ignoring the presence of an earlier grave. It follows from the same evidence that the Thamudic inscription, though not found *in situ* should not be considered intrusive, but should instead be not later than the cemetery."

This is an intentional latius hos which proves that the authors do not intend to respect objectivity because the slab is not a tomb and their respective position is entirely different ... and they know it. Furthermore, the first sentence lacks archaeological evidence because they admit that "several graves were crowded together" (p.15); and they cite three cases where a shallow tomb was "impinging on its [= of the first tomb] shaft" (p. 17). Is that not examples of "intrusive tombs"? Moreover, the authors admit that (p. 17) "we cannot rule out the possibility that these graves were reopened shortly after a first funeral." Is that not another example of "intrusive" initiative? The authors also claim that their theory of "simultaneous or nearly simultaneous burial" is proven because "Otherwise the disarticulated bones would have been collected as shown below for other cases." This is a specious reasoning because they do not, and cannot, prove that the second method was strictly obligatory, viz. that those people had first to remove the disconnected bones if they wanted to reuse a tomb. Moreover, even were the authors' initial statement correct, "the same evidence" would still not be applicable to the Tham slab for the putting of such an inscribed stone ON a tomb does not disturb the latter at all. Finally, their last remark is another irrealistic wishful thinking because they have no proof at all that the Tham slab was at the same level as the tombs; on the contrary, all details available so far suggest that that Tham slab was found in the top soil, viz. at least one meter above the cemetery level, and outside the western edge of the cemetery.

(e) The identity of the dead persons.

((1)) The basic consideration to be underlined is that not a single bone was put through scientific test to determine its age. It, thus, remains unknown whether the skeletons belong to the B.C. or A.D. era.

((2)) No inscription was found inside a tomb or at the cemetery level. Therefore, the ethnic group to which that transient community belonged remains unknown.

((3)) On the sole basis of Röllig's erroneous translation of "s^{c}dn" as "helpers" (cf. below), E.A. Knauf suggested to the authors that

> "The Arabic here has no clear meaning unless it is a direct translation of the Latin *auxiliares*, i.e. cavalry (Knauf: personal communication)."

((a)) What an imaginative latius hos! The very first thing Knauf should have done is to check Röllig's study and to find out whether or not his translation was justified. Whether he did it or not remains unknown. What is certain is that he blindly endorsed it; and his imagination was set in motion

to explain a fanciful translation. Knauf does not explain how some of those men could have joined the auxiliares of the Roman army even taking for granted - which is not at all proven - that their country had already been conquered by Rome. The suggestion that they might have been foederati is not better because the ethnic affiliation of those men remains unknown and the foederati came from large ethnic entities bound by treaty to Rome. Rome never bothered to sign a treaty with such small a community. More appealing would have been the suggestion that some of the dead might have served in one or another capacity in the impedimenta of a Roman legion.

((b)) Any connection with a Roman legion also is imaginative: not a single object belonging to a soldier was found with the skeletons. The authors claim that "There is some evidence in the graves for relations with the Roman armies" (p. 40), and they claim that the interred people "were in contact with the Roman troops in the area, as attested by the *caligae*."

Thus, the most important "evidence" is the "caligae" which were discovered in 5 tombs, viz. 14, 107, 111, 123 and "three pair in Grave 103A" (sic!; p. 20). But, nowhere do the authors raise the question: Why three pairs of shoes in a grave containing one skeleton only? It seems reasonable to conjecture that the man involved was the leatherworker of the community and that he was interred not only with the remaining samples of his craftwork but also with some leather leftover of his trade (cf. p. 85 under no. 103A and pl. 43B). Furthermore, one of those persons "was of a small child, presumably a boy, too young to be a soldier (Grave 123)" (p. 40), and p. 89, under no. 123, specifies that the skeleton belonged to "One "Small" "7 years"." I wonder how the authors know that that child was "7" instead of either 6 or 8 years old. This is a good example of the authors' aprioristic argumenting method. The "presumably" comes from their preconceived idea of "soldier" as if it could not as well be the skeleton of a girl. Therefore, those so-called "caligae" were also made for children ... presumably for them to play war games with sticks and pebbles! Nothing prevents the reader from thinking that those sandals were either made locally or even obtained from passing-by merchants.

Other examples cited by the authors are the leather wrapped around two persons, one of whom is the dead of tomb 103A (cf. above), and leather sheets because "These were not appropriate for normal use, but would have offered some protection for the animals against arrows." The word "animals" refers to Knauf's suggestion of "cavalry" (cf. above). But, if that leather stuff could protect "animals against arrows," it certainly could also protect human beings not only against arrows but also against the bitter cold and the sharp evening winds.

The authors' aforesaid explanation can hardly mean anything but that those leather sheets were intended to protect the horses, viz. those people must have used them as carapaces as in the Middle Ages! Such an idea logically comes from the fact that "We have traced the elaborate designs of the leather as far as South Arabia" (p. 39), which is a reminder of p. 20: they refer to the slab horse of the Dumbarton Oaks Collection. Thus, surprisingly enough, the authors jump from the saddle of this slab horse to carapaces and even further because the aforesaid phrase of p. 39 is immediately followed by this one: "The Proto-Arabic seal, incidentally, is also probably from Southeast Arabia and is certainly an Arab product, though much earlier" (p. 39). It is quite easy to throw such a phrase at the head of the reader without even a single word of explanation as to why, when, where and how. Their imagination is like a powerful magnet attracting everything they can think of: they are not even able to prove that the slab belongs to the cemetery but their imagination put it in anyway.

((c)) The authors also state that the dead of the cemetery "were in contact ... with urban centers where the finer objects were produced" (p. 38). The possession of all those "finer objects," which obviously enough had to come from outside, does not at all prove that those people had to be "in contact ... with urban centers" because those objects - also obviously enough - could very easily have been bought through the caravan trade. Of course, this is an indirect "contact" with the outside world, but not that which the authors had in mind.

The authors' whole commentary on those items is specious and presented for the sake of the cause, viz. Knauf's imaginative speculation on Roman armies.

(2) Röllig's second statement is as follows: "We may assume that the text was damaged when the stone was bulldozed" (p. 43).

(a) This statement is contradicted by the fact that no fresh break is visible on the photos. Yet, Röllig apparently needed R.L. Gordon to tell him what is evident from the photos. Incidentally, this detail is the only recorded instance of Röllig's contact with the world outside his ivory tower. Then, Röllig concludes as follows:

"Therefore, I assume that no part of the text is missing" (p. 43).

The only "therefore" there is is that his deduction from either his myopia based on incompetence or the formulation of an evidence is expressed by the same verb "assume;" viz. Röllig does apparently see no difference between a fact and a sup-

positions! But, toward the end of his commentary on his "blan (?)" of his text B, Röllig forgets all about his preceding statement of "Therefore, ..." and he goes back to his initial affirmation of "We way assume..." by writing that

"The possibility, however, cannot be excluded that the stone suffered some damage at its upper edge" (p. 44).

His new about-face is prompted by his inability of reading the end of his text B. Such an attitude is ample proof that Röllig may distort the epigraphical evidence to save his incompetence.

(b) More needs to be said on the subject. Gordon's testimony, such as released by Röllig, reads as follows:

"all the breaks are earlier than the inscription ... This is shown by the patina of the stone, which is found even in the incisions of the inscription." (p. 43).

These two statements are but a repeat of these two following affirmations of p. 34: "What appear to be breaks on the main inscribed surface are natural irregularities marking portions of the surface which project slightly more than others. Otherwise the entire stone is covered by a rusty patina which continues in the inscribed cut."

No reference to any part of the volume is given. But, Gordon should have explained the contradiction between his first testimony and a note of the "Museum Listing," a section of the book he himself coauthored and which contains the following information: "Surface flaking, two signs recently lost" (p. 54, in 13496).

This note is useless because (1) the exact location of that superficial chip is not pinpointed; (2) its measurements are not given; (3) the authors fail to explain how they know that there was some engraving there since the so-called flake is lost; and (4) they do not explain that exactly two "signs," which means "letters" in their vocabulary (cf. their expression "Two inscribed signs lt" two lines above), are missing instead of either one letter + one wasm or one two-section wasm. Furthermore, Gordon's preceding position is also contradicted by the note which immediately follows his remark of p. 57, viz. "Rusty patina overall and in inscribed cut," which reminds the reader of his first quotation reproduced above. If a "rusty patina" is "overall," there cannot be any fresh break.

(3) Röllig's third statement is

"At the upper edge of the stone, however, we have indications for a second author." (p. 43). The expression "the upper edge" is correct if one looks at pl. 35 as it is printed, without rotating the book. In fact, however, the caption is printed on the wrong side when compared with the correct position in the facsimile of p. 53 (cf. already above, p. 259A). Thus, the expression "the upper edge" must be corrected to "the left side" of the slab. Furthermore, what is the justification for his "second author"? Here again, Röllig limits himself to a mere statement. If those two letters, "lt,"

are compared with the five other groups of lt (cf. facsimile above, p. 259), no stylistic difference between the six groups can be detected; therefore, Röllig's theory remains unjustified and unjustifiable. It could easily be a false start, text c (cf. below), of the main text, as is the initial group of the same inscription, text b.

(4) Röllig's fourth statement is to the effect that

"As far as the main text is concerned, it derives from one author, but consists of two inscriptions." (p. 43).

If there is only one author, as proven by the palaeographic homogeneity of the whole group, there cannot be two independent texts running side by side; it has to be a single inscription composed of two or more sections.

(5) Röllig's fifth statement is as follows: [A] "It is difficult to state from which site inscribing was started. It seems that the [B] longer inscription (B) had to take account of the shorter one (A) and was forced to turn at a right angle. [C] Therefore, B may be secondary."

Ad [A] – What Röllig has in mind is the relative chronology of his sections A and B.

Ad [B] – His reasoning is a childish babbling. The engraver of his B curved the left end of his l. 1 upward and, then, rightward without any apparent reason since he could easily have engraved four more letters before curving his line upward. Similarly, he could also have made his second curve (on the right) upward to parallel the beginning of his text. Furthermore, it can easily be stated contrary to Röllig's solution that the engraver of his A had to alter the aslant (to the left) direction of his line to curve it to the right because of the beginning of his B. Moreover, the structure of the text is clear, viz. a short pedigree giving the names of both the engraver and his father is followed by three requests to the goddess Lat, and the first two elements of each petition are w + dkrt. It does not make any sense to isolate the last two wdkrt phrases because of the fallacious pretext of an inexistent "nearly cruciform dot" (cf. below). The preceding facts are so obvious that Röllig's reasoning once more proves how superficial and incompetent his work really is. In fact, the 59 lines of his commentary never explain his position; his stand is all the more surprising in that even the "Museum Listing" speaks of (p. 57)

"four connected lines of Thamudic writing: two lines parallel along end of face; two parallel near long edge at right angles to these."

The preceding information does not mean anything to Röllig; he most probably never read them either before or while writing his paper. Yet, that information

seems clear enough in spite of the awkward wording. Of course, Röllig does not refer to that statement, isolated, as he was, in his ivory tower.

Ad [C] – The adjective "secondary" must be replaced by "posterior."

(6) The sixth and last statement of Röllig's introduction reads as follows:

"Both inscriptions are "Thamudic" according to their script and arrangement; both are written *boustrophedon*."

If the second phrase is intended to explain the word "arrangement," it deals with half of the reality only. The position of the lines, horizontal or vertical, has nothing to do with their being boustrophedonic. Furthermore, his B is not boustrophedonic: it is composed of 15 letters making a more or less straight vertical line + 2 letters making a right angle with the preceding series. Finally, neither the position of the lines nor their boustrophedonic ordering justify the ascription of his A and B to Tham. As a matter of fact, they are much more common in Saf than in Tham. They are Tham because of the heavy-set ductus and the form of the letter d.

c – Röllig reads his A and B as follows:

"**Inscription A**

lm'bdt wdkrt lt bln

"For *tm'bdt*, and may Lāt remember *bln*." " (p. 43).

"**Inscription B**

wḍkrt lt kl lgy'n w'bdgn whtn knw s'dn wḍkrt lt b zz lsn.

"And may Lāt remember all (men of) *lgy'* and *'bdg* and *htt*. They have been helpers. And may Lāt remember *blsn* (?)." " (p. 44).

(1) Preliminary remarks. – It should be pointed out that the summary of p. 34 notes that

"As W. Röllig shows (Appendix A), the inscriptions represent more than one dedication to 'Allāt."

Even Röllig's texts mention three supplications to the goddess, but not a single "dedication," and the name "'Allāt" does not exist in Tham.

(2) Röllig's commentary consists of a series of confusing remarks and/or suggestions and of silly or erroneous reasonings, all proving – as his introduction had already done at length – his gross incompetence in dealing with an ordinary Tham inscription. It would really be a waste of time and paper to redress every section of such journalistic a verbiage. I chose as an example of Röllig's prose the three basic statements of the longest section of his commentary, viz. ll. 27-31 of p. 43, which deal with the sixth letter of his A; he writes as follows:

"Paleographically, there is no doubt that it is a *d*, but we may assume that it is a mistake for *d*. Inscriptions do contain mistakes (Winnett and Harding 1978: 16f), and *d* for *d* is already attested in inscriptions which belong to the same sub-group of Thamudic as the inscriptions which concern us here (Littmann 1949: 173-175; Knauf 1983: 588 § 2.3.2). *'bd* occurs frequently in Ancient Arabian personal names."

(a) Here again, the reader notes Röllig's favorite expression "we may assume."

(b) Here again, Röllig does not justify his assumption. But, his paper makes it evident that his reasoning is as childishly simplistic and erroneous as can be: he reads the name as "tmʿbdt" and, at a loss to understand his own decipherment, he salvages his ignorance by falling back on the well-known name ʿbd and, thus, he decided that d HAD to be an error instead of d; consequently, the name becomes "tmʿbdt" in his translation (note also the elimination of the ° above the letter "b"); then, he embarked on a search for a postmortem justification of his ignorance.

(c) The existence of engraving errors is a fact. Such errors, however, must be obvious or apodictically proven. The unreserved respect due to any text imposes upon all researcher the sacred duty of taking the text as it is, thus making it plainly unscientific the mere assumption of an error in order to please oneself or to fit into one's thesis or to hide one's incompetence or to elude a difficulty.

(d) Since Röllig refers to WiHaI, p. "16f," let us take in p. 16 a short series of so-called errors, viz.

"Errors with no attempt at correction are as follows: wwwd for wwjd or wwdd (23), ʿḫ for ʿl in 116(p), bklt for bkrt in 148a(p), ʾšršq for ʾšrq in 589(p), bʿdy for sʿdh in 695(c), rlh bt wʾblh for zrlh bn whblh in 1425(c); mqmlʿ for mqmʾl in 1312."

Thus, the WH texts involved are, respectively, 23, 116, 148a, 589, 695, 1425 and 1312. The study of those cases also proves Röllig's gullibility.

((1)) No photo of the first four texts was released in the edition, and the availibility of the photos of the WH collection is restricted the main editor's friends, viz. those who flatter him. Note that the parentheses of the first example, "(23)," may be explained by the fact that the transliteration of the text (p. 39) retains "wwwd" only; the mention of "wwjd" is found only in the commentary. Furthermore, the siglum "(p)" must be added after "(23)" because the publication of the inscription refers to "(pl)" which does not mean "(plate)" but "p[hoto] 1;" the photo is not to be found in the plates.

(2) Even the editors themselves do not have any photo of the last three texts which, thus, are known through their copies only.

Consequently, WiHaI's copies of those seven texts cannot be checked. No true scholar will ever use any of those WH copies to justify the important decision that there was an engraving error in any of those inscriptions. The following considerations on those copies make it clear that the editors' opinions are wholly unreliable.

[a] The editors' reading of "wwwd" in WH 23 is erroneous; their own copy has wwdd. It is manifestly dishonest for anyone to accuse the engraver of an error which exists in his decipherment only.

[b] The editors' reading of their copy of WH 116 also is erroneous; cf. JaMAR, XIII, pp. 198-199.

[c] The editors shamelessly contradict themselves on WH 695 because their commentary (p. 146) states that "b^{c}dy must be a mis-copy of s^{c}dh," viz. they acknowledge that they miscopied the original. Therefore, they accuse the Saf author of an engraving error on p. 16 while acknowledging that it is their own mistake in p. 146! The same Saf text WH 695 also illustrates another kind of error deriving from such misguided a chase of engraving errors. The editors declare a ṯ to be the engraver's error instead of ḏ and, in so doing, they eliminate rṯy, a secondary form of the divine name rḏw; cf. rṯy = rṯw = rḏw = rḏy = rḏ^{3} in Orientalia, 48 (1979), p. 518, and JaMAR, XIII, p. 272A.

[d] The editors' understanding of WH 1425 also is erroneous; cf. JaMAR, l.c., p. 207.

[e] The editors' reading of "mqml^{3}" in WH 1312 (as in their facsimile) undoubtedly is possible and cannot be corrected to "mqm^{3}l" without the proof of a legible photo, and no photo does exist. The qtl+l^{3} nominal derivation does exist; cf., e.g., s^{c}dl^{3} of JaS 47.

(e) In order to prove his borrowed theory of d = ḏ, Röllig refers to two publications.

(1) In Röllig's reference to LiNAD, "pp. "173-175", the pagination is erroneous; pp. 173-174 and 174-175 deal, respectively, with "ث steht für ت" and "ت steht für ث." It does not matter where Röllig copied his reference, but it matters that he did not read Littmann's pages 175-179 which deal with "Die Laute ʾ, ʿ und ʾ." To admit that Röllig actually read Littmann's study would indeed make his case much worse because it would entail either that he did not understand a text written in his native language or that he voluntarily distorted its testi-

ony.

(a) LiNAD's first statement is as follows (p. 175):

"Wenn in der Inschrift Harding Nr. 490 ZT steht für *Zaid*, während derselbe Mann in der daneben stehenden Inschrift 489 ZT heißt, so ist ZT wohl nur ein *lapsus calami* und läßt zunächst nicht auf die Aussprache von ذ als د schließen."

Littmann's statement is based on his inaccurate study of the original. HaLi 490+489. – HaLiSTI's facsimile (pl. 22) and reading of the two lines of the inscription (p. 45) are as follows:

" 489. *w ḏkrt lt zd bn šʿr ḏ ʾl ḫlʾl* وبكرت لت زد بن شعر ذ ال خلال

And may Allāt remember Zaid son of Shaʿir of the tribe of Khill-ʾEl.

The form of the *ʿ* is most curious, but we have had *šʿr* before in 174, 190 and 214, so it seems reasonable to read it here; 490 repeats the name. See 323 for Zaid; Khill-ʾEl is Safaitic and Thamudic.

490. *w ʾ d z ḏ ḫ l ḏ w(?) r w'lt bn šʿr ḏ ʾl ḫlʾl* وء د ز ذ خ ل ذ و ر وألت بن شعر ذ ال خلال

The letters of the first part of the text do not seem to compose into words, and have, I fear, been miscopied. This is clearly a brother of 489, name probably "Wāʾilat son of Shaʿir of the tribe of Khill-ʾEl."

[Probably ... وجد زد كل حير ولت "Zaid (د mistake for ذ) found every young camel of Waʾilat ....." Because Zaid found the camels he is blessed in No. 489.]"

The two lines form a single inscription: they are perfectly parallel to each other; they show the same palaeographic ductus; and they are characterized by three sets of two dissimilar forms each to represent the same letters, viz. ⊖ ⊟ for *w*, I ⅄ for *d*, and ⅂| for *l*. Both Harding and Littmann read I as "*z*" instead of *d*; the second and fifth letters of no. 490 are correctly read as *ʿ* and *d* by Harding, but incorrectly as "*g*" and "*d*" by Littmann because of his gratuitous statement of "(ذ mistake for ز)." The second letter of the name *wllt* is erroneously read as "ذ" by Littmann. The second letter of the name of the father of both *wllt* (no. 490) and *dd* (no. 489) is *š*, not "*r*" as in the edition, or "*ṯ*" in JaMAR, IV, p. 138, note 83. For that form of the letter, cf. my genealogical tree of Saf *š* in my paper on "Safaitic Inscriptions from the Country of ʿArʿar and Raʾs al-ʿAnāniyah," in F. Altheim und R. Stiehl, Christentum am Roten Meer, vol. 1, Berlin, 1971, p. 612, *š*, nos. 14 and 16. Finally, Littmann's understanding of the text is quite unusual. Contrary to the normal style of the inscriptions, it is his "Wāʾilat" who is considered as the author and engraver of the two lines although the name of that person is only mentioned incidentally

in no.490. Furthermore, no. 489 obviously is an imprecation to the goddess Lat, and it is that imprecation that Littmann surprisingly interprets as "he is blessed. The text may be read as follows:

1 wʿd ddkl ḥwr wʿl,lt bn lšr dʾl hlʾl
2 wdkrt lt dd bn lšr dʾl hlʾl

1 Wâʿid, he of Dâkil, has made Wallat, son of Ilšâr, him of the tribe Haylʾil, to return;

2 and may Lat remember Dawd, son of Ilšâr, him of the tribe Haylʾil.

L. 1: wʿd, e.g., Madaba 297b. – dkl: for the first element of this qtll (l = il) nominal derivation, cf. JaT 104c. – ḥwr, cf. Ar ḥâra (o), 2nd form "to make someone/something to return" (cf. LaAEL, p. 665B). – wllt, a qttl (reduplication) derivation; cf. Ar walata "dimicuit, imminuit, defraudavit aliquem" (cf. FrLAL, IV, p. 502B). – lšr: a l (= il)qtl nominal derivation; cf. Ar šâr = šîyar "consulter, counseller with whom one consults" (cf. LaAEL, p. 1616C and 1617B). – hlʾl, cf. the personal name in Hu 241b.

L. 2: dd, HaLi 323.

The message of the text seems to be as follows. A certain Wâʿid made Wallat, son of Ilšar, to return because Wallat's brother, Dawd, had gone away definitively. Therefore, Wâʿid must have been some kind of administrator of a family, and he made the new family chief to return to take over his brother's succession.

((b)) LiNAD's second statement concerns the divine name dšr = dšr. His opinion is so clearly expressed that no comment is needed. It reads as follows (pp. 175-176):

"Auch hier ist nicht ohne weiteres auf einen thamud. Übergang ذ > د zu schließen; denn דשר ist die fremde aramäische Form, דשר die einheimische Form bei Safaïten und bei Thamudenern, und beide Formen haben nebeneinander existiert."

((c)) LiNAD's third statement is to the effect that (p.176):

"Die Namen רדאל, Nr. 109, und ירד, Nr. 255, sind mit ذ nach dem Arabischen schwer zu erklären. Vielleicht steht hier ד für ד (wie in זד, oben S. 175). Das würde auf ein Schwanken der Aussprache zwischen ذ und د deuten."

Littmann's opinion is as erroneous as his pan-Ar which forces him to evaluate the derivation of NA names, theophoric or not, on the sole basis of Ar onomastics. G. L. Harding (cf. HaLiSTI, p. 19) had already referred [illegible] to Ar [illegible] cf. "[illegible] or light rain," and it was Littmann who again and without giving the slightest justification added this remark to Harding's commentary: "[Perhaps Radd-ēl 'God has restored', if ذ stands for د.]." In both publications, however, Littmann introduces his erroneous opinion with "vielleicht" (1950) of "if" (1952).

((d)) LiNAD's fourth and last statement reads as follows (p. 177):

"Ähnlich steht es mit dem Namen ידע, Nr. 69. Er könnte von der Wurzel ذاع abgeleitet sein, ist mir aber als solcher nicht bekannt. Eher ist an ידע zu denken, das im Safaït. als Name belegt ist."

Littmann was apparently so confident in his omniscient knowledge of Ar that he felt authorized to discard the root ذاع because it "ist mir aber als solcher nicht bekannt," whatever the meaning of such cryptic an expression may be ... as if he did not have at hand dictionaries where he could have learnt that the root ذيع does in fact exist; cf., e.g., LaAEL, p. 990A and FrLAL, II, p. 101A.

Summarily, Littmann examines five names, and his answer is negative and dubitative in the first two and the last three cases, respectively. But, according to Röllig, Littmann's answer always is affirmative in favor of his d = ḏ!

((2)) Röllig's second reference is E.A. Knauf in ADAJ, 27 (1983), p.588B, par 2.3.2. To prove that "ḏ steht für /d/," Knauf refers to three names; his text reads as follows:

"ذ steht für /d/ in زذ TIJ 490; in يذع TIJ 69; in يرذ TIJ 255 (زد, يدع und يرد sind gut belegt)."

The simple fact that zd, ydʿ and yrd "sind gut belegt," as Knauf puts it, does not, and cannot, prove that their reading must be found at all cost. Furthermore, Knauf does not acknowledge that he plagiarizes Littmann and, in the process, he gives another example of his peculiar argumenting method. Indeed, if the name yrḏ is a valid example for his thesis, why does he omit rḏʾl mentioned by Littmann with yrḏ? Knauf's apparent justification is that the name rḏʾl, being an hapax legomenon, is not one of those names which "sind gut belegt." It is ridiculous to base a consonantic equation of the basis of names which "sind gut belegt."

Summarily, Knauf plagiarizes Littmann whose errors he considers as certainties and whose sound arguments he sweeps under the rug; and in the process, he adds two erroneous reasonings. Of course, Röllig is free to endorse whosesoever argumentation he likes!

(f) Röllig's remark to the effect that "ʿbd occurs ... names" (for his quotation, cf. above, p. 271) is

a useless truism since the name does not exist even in his own transliteration.

d – The extent and value of each text.

(1) The unicity of JaT 131a.

(a) Röllig's remark of his introduction (cf. above, p. 269) explains nothing and is unacceptable.

(b) Röllig's second remark on the subject also is a mere statement; it reads as follows (p. 44):

" A larger, nearly cruciform dot is used as a sentence-divider."

There can hardly be any doubt that, in Röllig's mind, his statement justifies his splitting of the text into two independent inscriptions. The so-called "sentence-divider" is Röllig's invention. What does exist is signs, bars or dots, which either separate letter(s) or word(s) inside a text or introduce or follow an isolated inscription. Moreover, there is no "nearly cruciform dot" even in Cowherd's facsimile (cf. above, p. 259A), and the dot drawn there in her facsimile is a misrepresentation of the original sign which is a short straight line. Finally, to the left of the vertical l Cowherd's facsimile shows a small dot which also is a misrepresentation of a short horizontal line which is paralleled by another straight line above (cf. above, p. 259B). These two lines are other features of the stone having nothing to do with the text.

(c) One example of the many awkward, unintelligible expressions used throughout the "Museum Listing" (pp. 57-70) is this: "Lines:

End Set: L. 15.0
Edge Set: L. 18.6."

The preceding information is supposed to contain the length of Röllig's A and B. The word "set" indicates that the authors refer to the two, horizontal and vertical, groups of two lines each. But, such a division does not fit Röllig's understanding of the lines. Furthermore, where is the "end" and where is the "edge"? Both sets are on the edge of the slab and have two ends each. Therefore, which is which? The only indication available is the given measurements. If "15.0" is the height of the vertical group of pl. 53, "18.6" becomes "16.3" for the other set!

(2) The second false start of the text = JaT 131b. – Röllig writes the following statement on the question:

" It is worth noticing, however, that *lt* is repeated to the left of the first letter, incised by means of an instrument that was split and produced double lines. It is impossible to say whether these two signs belong to inscription A or B, if to either of them; therefore, I assume that the scribe started to write with an inappropriate instrument, stopped, and started again to the right side of the two first letters, and completed his inscription."

Röllig did apparently not know what to do with the two letters located to the left of the upper extremity of his A, and his expose is confused because his second statement is contradicted by the third, and the latter actually contains the true solution which he did not realize as such. The situation is simple. The instrument used to trace those two letters was not "split;" the letters belong to the double-tracing process which was abandoned during the engraving of t since its vertical element is composed of one line only. The two letters are a false start of the text, which the engraver started all over again on the right.

(3) The feature on the far left: a possible first false start of the text = JaT 131c = RöTI C.

Röllig's information on that feature reads as follows (p. 45):

" Inscription C

At some distance from the main texts, inscription C is to be found at the left edge of the stone. This inscription consists of two letters only: *lt*, that is the name of the goddess (Al)lat."

The expression, "At some distance from the main texts" and "at the left edge of the stone" are remarkably vague and useless, respectively. In Cowherd's facsimile (cf. above, p. 259A), the letter "C" should have been added since "A" and "B" are there; the location indicated by the arrow is even more approximative than those given by the two arrows which are even farther away from the level of each section of the text; and the broken line of the arrow remains unexplained. In fact the facsimile is useless. Since p. 45 contains only the aforesaid quotation, it is almost ridiculous to state that the accurate information on those two letters could easily have been added to the present commentary; e. g., the dimensions of the two letters, and the distance between them and both the lower left corner and the left side of the slab. But, to make things worse, Röllig's expression "at the left edge of the stone" equals "at the upper edge of the stone" (p. 43; cf. above, p. 268) and also " at the centre of this part ," which is the "recessed" "face" (p. 57. What can be seen there in the photo is a small cross and, above it, the aslant edge of a feature of the stone. Therefore, if t alone exists, it is an isolated wasm without any connection with the main text. If, however, the two letters "lt" do exist, they may safely be interpreted as the first false start of text a.

3 – My reading of JaT 131 a = RöTI A+B is as fol-

lowe ltmʿ bdt wdkrt lt b	1	By Tamaʿʿ, son of Dât; and may Lat remember Ba-
ln	2	llân;
wdkrt lt kll gyʿ]n w	3	and may Lat remember the household servants of Gayʿân and
ʿb	4	of ʿAbd,
d gnw ḥṭṭn knw s	5	[who] have concealed Ḥaṭṭân, [and] have sheltered Sa-
ʿdn wdkrt lt wh	6	ʿadân; and may Lat remember Waha-
al ʿnn	7	mil, [who] has appeared [here].

As already stated above (p. 269), the text is composed of a short pedigree followed by three imprecations to the goddess Lat, the beginning of each of those requests being the verbal expression wdkrt lt. The use of a single verbal expression followed by all the names as, e.g., in HaLi 58, would have made very difficult to accurately specify the subjects of the two asyndetic verbal phrases introduced by gnw and knw, respectively.

L. 1: tmʿ: Ramm 9. – dt, e.g., WiAM 18. – dkrt, e.g., Madaba 507.

Ll. 1-2: bln, e.g., Hu 181.

L. 3: kll, cf. Ar kall, pl. kulūl "domesticus, qui alitur; domestici, quos quis sustentat" (cf. FrLAL, IV, p. 49B, and also Wörterbuch der klassischen arabischen Sprache, Wiesbaden, I [1970], p. 292A-B). – gyʿn, cf. gwʿ of JaT 38g.

L. 5: gnw, pl. of gn, for which cf., e.g., Hu 256. – ḥṭṭn, cf. ḥṭṭ in Saf, e.g., CIS 4791, and in Liḥ JaL 108 d/2; ḥṭ in Tham JaT 77, in Liḥ JaL 8ab, and in Saf, e.g., in JaS 172b; Tham ḥṭl in Eut 568, and Saf ḥṭn in CIS 1231. – knw, pl. of kn; cf. Ar kanna "texit rem, custodivit puellam" (cf. FrLAL, IV, p. 60A, and also Wörterbuch der klassischen ..., l.c., p. 370A), and also, e.g., in VoEAI-T 1.

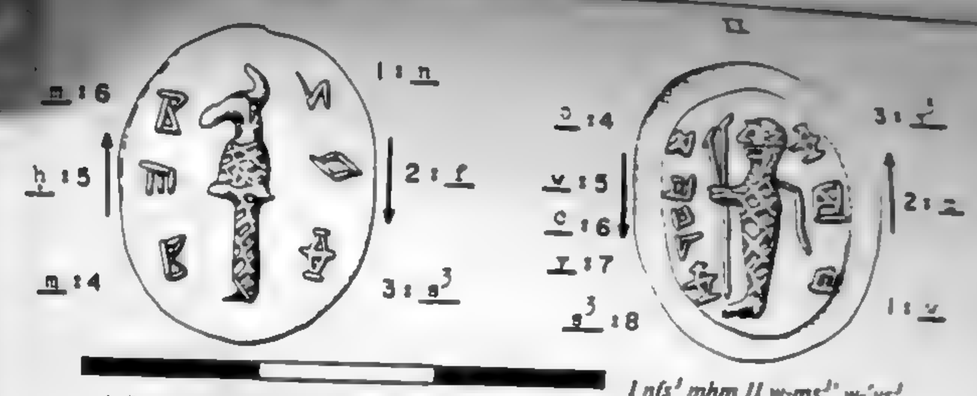
Ll. 5-6: sʿdn, e.g., Hu 670.

Ll. 6-7: whml, a qtll (l = il) nominal derivation; cf. Saf whmn in CIS 865, and whm, e.g., in ScPNO 80.

L. 7: ʿnn, cf. Ar ʿanna "to appear, show, present, offer itself" (cf. LaAEL, p. 2162C).

C – The inscribed Ḥaḍr seal Ja 3204 (cf. pp. 22-23 and E.A. Knauf, pp. 46-47, pl. 37, central horizontal line of photos, and pl. 54, facsimiles).

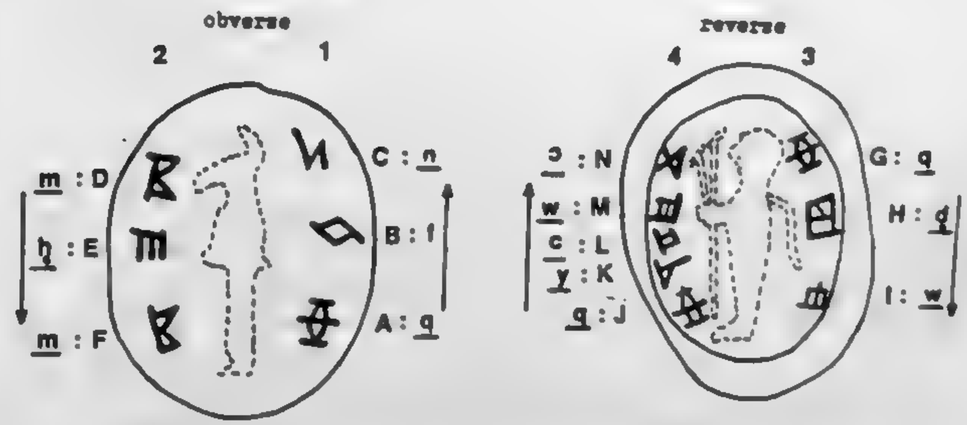
1 – Preliminary remarks. – The caption "Appendix B:



1. Proto-Arabic Seal of Green Stone 20/5/44 Appendix B

I nfs³ mhm II w-ms³' w-'ys³
Seal (?) of MHM, MS' and 'YS

Facsimile of the seal KnPAS by "Mr. Robert Erskine" (pp. 10 and 14) = "R. Erskin" (p. 11) with Knauf's double numerotation (pl.54A) and his translation (p. 46). My two additions are the arrows and the value given by Knauf to each letter.



My sketch of the Ḥaḍr seal Ja 3204 = KnPAS. – The two figures of the seal are in stippling because the published photos are too dark and too fuzzy to allow the study of the details. The four vertical lines of the text are numbered 1 to 4; the arrows indicate the direction of the two pairs of boustrophedonic lines; and the fourteen letters are designated by the continuous series of capital letters, A – N, which are each accompanied by the individual value of the letter.

The Proto-Arabic Seal" (cf. also above, p. 258) is mistakenly printed above p.49, which contains the end of "Appendix C: The Shells." – Knauf's text never refers to the other sections of the book, except for his mention of pl. 37B and 54.

2 – Identification.

The siglum of the seal is "20/53/44" in pp. 22 and 46; and in this pl. 46 Knauf does not refer to the "Museum Listing" (p. 63) which gives the siglum 13544 along with the following measurements: length: 1.6 cm. width: 1.3 cm. and thickness: 0.25 cm. – Throughout the following pages with the exception of the quotations of Knauf's text, all references to the seal are in accordance with my tracings (cf. above, p. 279B); the reader will easily compare the latter with the documentation released in p. 46 and pl. 54, which is also reproduced in the same page 279 in A.

3 – Description of the seal in p. 22.

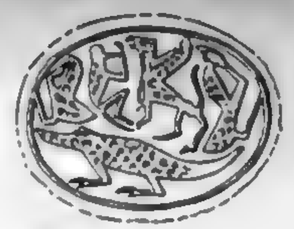
The inscribed seal is a "flat, oval, with beveled border" "dark green stone from Grave 20" according to p. 22. For further details, cf. above, p. 258, on pl. 37B. It is said that (p. 22)

> "The bodies of the two figures are similar, roughly but clearly cross-hatched to indicate the wrapping of a mummy."

It all depends, of course, on the meaning given by the authors to the adjective "similar." But, the first similar feature of the two figures is that they are standing on their feet. If the figure of the reverse is human, that of the obverse is mythical, not even anthropoid. Furthermore, the second common feature is that the checkered design, which does not concern the bodies themselves, adorns the long dress only of each figure and is not to be found on the following sections of the two figures, viz. the head, neck, arms and feet of the first, and the face, arms and feet of the second, all of which show no sign of being covered by any cloth. The back of the head and neck of the second figure are covered by the upper part of the long garment, but that upper section seems to be void of any checkered design. Furthermore, it is regrettable that the authors did not utilize the material published in the volume. Indeed, a checkered pattern similar to that seen on the seal also decorates "the scarab seal from Grave 75," 75/04/99 (pp. 22, 81, photos in pl. 37A, and facsimiles in pl. 51C) = "Museum Listing" 13499 in p. 57, where the description does not mention any design decorating the five figures. But, p. 22 has this on the scarab seal:

> "The cutting is sure, precise and vertical, producing sharp outlines and clear, regular hatching. The style, however, is relatively schematic."

This description does not mention any checkered pattern on the five figures be-



The facsimiles of the obverse of the inscribed seal (pl. 54A) on the left, and of the "scarab seal" (pl. 51C) on the right, showing the checkered pattern design.

cause "regular hatching" cannot be synonymous of "checkered pattern;" and it does not in any way allude to "the wrapping of a mumay." It is, therefore, flabbergasting that the checkered design means so little in the case of the five animals of one seal so as to be unworthy of even a mention, but indicates "the wrapping of a mummy" in another seal. Furthermore, the mummification involves the whole of the body, leaving no part aside or outside. That checkered design appears to be a decorative ornament. Finally, note also that the reader has to go to the beginning of the book to find out that the facsimile of the seal with its numerotation of the letters according to Knauf's reading is the work of "Mr. Robert Erskine" (p. 14) or "R. Erskin" (p. 11).

4 – Photographic documentation.

The four photos of pl. 37B are of inequal quality. The two photos of the left are practically useless; the two of the right are too dark and fuzzy. The captions, "Face I and Impression" and "Face II and Impression" under the two photos of the left and the two of the right, respectively (cf. also. p. 9), are erroneous: the two photos of the left are those of the original. Therefore, the two captions should be corrected to read "Faces I and II" on the left, and "Impressions of Faces I and II" on the right.

5 – Palaeography.

The palaeography of the seal, which is not even mentioned by Knauf, is characterized by four facts.

a – The vertical axis of the normal stance of five letters, viz. A = C = J, K, L and N, is perpendicular to that of the line; and that axis of five other letters, viz. C, D = P, E, H and I = M, is in the axis of the line. The reason for such a distribution is not immediately apparent and mostly depended on the worker's choice. It may be noted, however, that the stance of B may be justified because it fills the space better; that of D = P may also be

explained by graphic elegance; and that of G, J and K by space limitation.

b – All letters belong to a uniformly angular (not "square," as in p. 22) tracing; and, therefore, "all the engraving was the work of one hand" (p. 22). The angular lettering is not unknown in SA epigraphy; cf., e.g., Ja 2338, 2341, 2346 and 2348b and c in JaMAR, VII, pl. 26 and 27.

c – With the exception of N, each letter contains at least one bar extended beyond its normal length. There are

one protrusion in C, F, H and K;

two protrusions in B, D, E and I;

three protrusions in L and M; and even

four protrusions in A = G = J.

The freedom of engraving enjoyed by the worker is illustrated, here also, by the double fact that the two pairs of letters, D = F and I = M, belong each to two different categories. The preceding palaeographic characteristic is also found in other seals or similar antiquities, and Knauf does not refer to any of them (cf. below, Appendix I).

d – The form of = N is very characteristic and is discussed below, in Knauf's basic statement.

6 – The direction of the lines.

The direction of the lines must be determined by both palaeography and the possibility of translation. L. 1 is read from bottom to top because C is senestrograde and, therefore, points to the next letter on the left, viz. D; and this information ascertains the direction of ll. 1 and 2. Furthermore, were l. 2 to be read from bottom to top, the horizontal bar of E would be supporting (thus, below) the three vertical bars. The palaeography of the three letters of l.3, G, H and I, is such that it gives no clue as to the direction of the line. This line, however, is read from top to bottom because the root wdq does not exist, and ʿdw provides a sound meaning for the whole text. Although K of l. 4 would fit the reading of the line from top to bottom better, the reason invoked in l. 3 is valid here also.

7 – Knauf's study (pp. 46–47, and facsimiles in pl. 54A; cf. above, p. 279A).

a – Knauf's overall interpretation of the seal is fantastic, to say the least, and his paper is composed of bias, unsubstantiated statements which often happen to be erroneous. But, first of all, nowhere does he justify his overall interpretation, viz. what is the rationale behind his thinking? In my opinion, Knauf's starting point is the story of "the wrapping of a mummy" (cf. above, pp. 280–281), which necessarily presupposes death. Then, the unexpected coincidence of the two letters nf/fn (l. 1) automatically brought to mind the well-known word nfs which, in turn, called for a name in l. 2. Finally, the interpretation of the two w's of ll. 3 and 4 as the conjunction "and" led to the interpretation of those two lines as containing two names. Even at first sight, however, such an argumentation is childishly simplistic. Yet, the 50 lines of his paper proceed along that line which, it must be repeated, is never justified or even stated.

b – The four pillars of Knauf's interpretation.

(1) Knauf's basic statement, on which his whole interpretation rests, manifests a distorting argumenting method based on a limited knowledge. The very first line of his paper contains a mere affirmation which reads as follows (p. 46): the seal "may be identified as Proto-Arabic." That his "may be" is an empty window dressing is proven by the fact that the title of his paper is "The Proto-Arabic Seal" without any question mark (p. 46), an expression which is repeated in pp. 5, 14, and also in pp. 9, 11, 47 (heading), pl. 37B and 54A without the article, and which becomes "proto-Arabic inscriptions" in p. 23, where "inscriptions" should be corrected to "letters." That expression, however, is not mentioned in the "Museum Listing" (p. 63, where it is given as "Green Stone inscribed Seal"). It cannot be emphasized strongly enough that the paper of Knauf, which contains 50 lines, does not even contain a single iota justifying his oracle. The importance of his affirmation makes its gratuitousness even more conspicuous and more baffling. Is it not a clear case of preconception which is the most unscientific approach to any question? Is it not an unnatural use of the human mind which will inevitably see, and see only, that which is the rut narrowly defined by the preconception blinkers?

As a consequence of his verdict, Knauf states that

" Within the Proto-Arabic corpus, this *alif* is without parallel " (p. 46).

Knauf's argumentation is the same as that of J. Ryckmans who, after excluding the Min rock inscriptions from the al-ʿUlā area from the Min corpus, claims that they indeed cannot be Min because they do not fit in that Min corpus! Knauf, however, acknowledges that I saw that form of in very archaic texts in Wādī Ḥaḍramawt (cf. BASOR, No. 172, December, 1963, pp. 41–54); and he is gracious enough to state publicly that he "tends to agree with" ... W.F. Albright's date of "between the

tenth and the eight centuries" B.C. In my opinion, this date is certainly too low my original date was and still remains about the 12th century B.C. for the first appearance of the letter. W.F. Albright preferred not to release my opinion so as not to ruffle the susceptibility of some of his friends who had embarked upon the mini-chronology. Furthermore, Knauf's ignorance is also proven by the fact that that form of is also found in many other rock inscriptions from Wâdî Ḥaḍramawt; cf., e.g. Serjeant-Ḥūd 2 in Le Muséon, 67 (1954), pp. 181 and 183; G. Caton Thompson, The Tombs and Moon Temple of Hureidha (Hadhramawt), Oxford, 1944, pl. 69, no. 59b = Wissmann 9b, l. 1 in Le Muséon, 75 (1962, p. 227; Wissmann 8b in Le Muséon, l.c., p. 226 (this rock inscription will be republished again at a later date); G.L. Harding, Archaeology in the Aden Protectorates, London, 1964, p. 52, nos. 8/2, 15/1, 16b, etc in RSO, 40 (1965), pp. 290, 291, 292, respectively; etc.

The peculiarity of this which is attested so many times in Wâdî Ḥaḍramawt amply justifies the opinion of the Ḥaḍr origin of the seal.

(2) The second pillar of Knauf's theory is his completely unjustifiable injection of the well-known word nfs into the text; but it is, in my opinion, the logic deduction from "the wrapping of a mummy" business.

(a) His overall thesis forced him to arbitrarily interpret = as "ś" (= his s^3), thus, inventing an inexistent letter in ancient Ma and also in contradiction with the value of ś. The consonant s always is rendered by š; cf., e.g., nps in Hebrew, Ugaritic and Phoenician, which equals nfs in Sa, Ma (cf., e.g., Lih JaL 103 i/2 and Saf AO 4968 a and b), Ar and Ge'ez. The preceding erroneous argumentation Knauf repeats it twice in his commentary on the letters G and J.

(b) From a palaeographical viewpoint, the letter is q; cf. in Ja 2338 (cf. JaMAR, VII, p. 26), and with the central bar extended through the central rectangle, or , e.g., in JaF 107/1 and Hu 503. Then, with the protrusions so widely added to the sides of all letters with the exception of M, (e.g., in HaLi 278) becomes . In J, , the upper square only was replaced by an isosceles triangle, and both extremities of the outside horizontal bars slightly protrude beyond their normal length (cf. above, p. 279B). In both A and G, however, the two squares were replaced by isosceles triangles. This is another proof of the liberty of engraving enjoyed by the worker. Finally, Knauf's facsimile distorts J by representing the two central vertical elements of the lower half as two salant strokes as in the upper half.

(c) Knauf is so blinded by his preconception of "nfs" that he does neither justify his reading of l. 1 from top to bottom nor even mention the possibility of reading the name in the opposite direction, viz. "sfn." But, of course, "sfn" (cf. Ar safana "to strip off, scrape off" in LaAEL, p. 1375B) does not fit his preconceived scheme based on "the wrapping of a mummy."

(d) The last remark to be made here is Knauf's transposition of letters; his "s^3" becomes "Ś" t h r i c e in his translation (cf. above, p. 279A), which he explains as follows:

> " The suggested translation is based on the observation that s^3 is over-represented in this text, whereas s^1 is completely absent. In those Semitic languages which distinguish s^1 from s^3, it is s^1 that is much more frequent. Therefore, s^1 and s^3 probably had merged in the dialect of the Alia-Seal inscription (as they did in later North Arabic, from the middle of the first millenium B.C., and in Ethiopic). In all other scripts where this happened, it was, however, the sign for s^1 that remained in use for $/s^1+s^3/$" (p. 46).

Such an argumentation is solely for the sake of the cause. When the two letters are assimilated, ś is no longer in use, and the simple fact that ś is still used proves that there was no assimilation yet. That ś "is over-represented" is a mere statement demanded by his erroneous and gratuitous injection of the word nfs into the text. And ... ś does not even exist!!!

(3) The third pillar of Knauf's thesis, viz. his interpretation of the two w's, I and M, as the conjunction and, is set forth as follows:

> "The stele was thought to represent the soul of the deceased. It is easily conceivable that a seal represents its owner or owners in a similar way. It cannot be excluded that the seal was jointly owned by three persons. It is more likely, however, that the seal was owned by three persons at different times" (p. 46).

(a) It is absurd to compare a stele with a seal and to use the verb "represent" to characterize the symbolism in each case. It is not "conceivable" that a seal "represent" even a person. A seal always is a personal thing which is not shared by anyone else except by delegation.

(b) There is no epigraphic evidence justifying the thesis according to which those ancient populations ever believed in the existence of the "soul;" and their fundamentally materialistic civilization leads into the opposite direction. If, in his statement, Knauf was referring to a meaning different from that which is ordinary, he should have explained himself or have used a word or an expression that left no

doubt about what he had in mind.

(c) The preceding statements present the author's reasoning which, based as it is on "nfs," forced upon him the interpretation of w as a conjunction. However, this interpretation of the two w's a priori is not even demanded by his arbitrary premiss on "nfs" and is, therefore, another whim of "la folle du logis." Indeed, it would have been natural to find in the balance of the text a short summary of the personal life of the owner, his qualifications or his achievements. But, such a deduction is positively excluded by his overal interpretation which is explained below.

(d) To suggest that a seal could be "owned by three persons at different times" is absurd because a seal basically is a personal property. Knauf's theory is even more absurd because the second user would keep on the seal the name of his predecessor, and the third ... the names of its past two owners!!! Such a theory is nothing but a tall yarn.

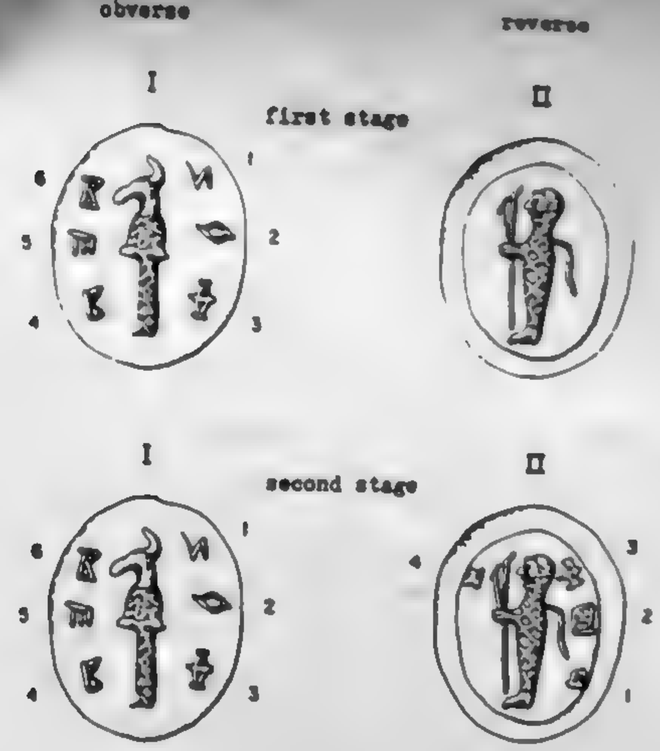
(e) Knauf did apparently not realise the manifest difficulties created by his understanding of the seal.

(1) How was Knauf able to find out that the first owner knew for sure that his seal would be owned later on not only by one but by two other persons and that those persons would engrave their names on it? What a superhuman foreseer and an unusually thoughtful person that so-called first-owner had to be to leave blank both sides of the reverse of his seal to accommodate his two followers? The same remark is equally valid for the so-called second owner.

(2) How did Knauf know that his three persons, who are not even presented as belonging to the same family, did own the seal during such a short time and live at the same spot so as to make it possible for the three of them to utilize the service of the very same engraver?

(3) From a stylistic viewpoint, the reverse of the seal during the first two of Knauf's three stages would be each a manifest misfit illustrated by the obverse (cf. the following page). In the first stage, the figure would be flanked by two blanks which would indicate that something was missing on both sides. In the second stage, the reverse would be even uglier than in the first because of the uneven distribution of the letters leaving most of the left side blank although the four letters could very easily have been engraved on the right side.

(4) Knauf's fourth pillar is his erroneous reading of H as "n." Knauf is in good company: F.V. Winnett read



The three stages of the utilization of the seal according to Knauf, who states that

« It is more likely, however, that the seal was owned by three persons at different times" (p.46).

the same 𐩣 of Ṣa'ūil 51 as "(a)" in Berytus, 22 (1973), p. 70; for this text, cf. JaMAS, XXII, p. 158. This letter 𐩣 is so different from the m's of D 𐩥 and of P 𐩥 that it is impossible to either confuse or assimilate them. Yet, Knauf succeeded in doing just that. The short aslant bar in the center of the upper half of the letter most probably belongs to a false start of a senestrograde 𐩭 of either y or m. However, the left vertical and the central horizontal bars are sure proofs that the engraved letter is ḏ 𐩣. Since the letters are cut in, the worker could not possibly erase the material proof of his unfortunate distraction. Finally, it is inexcusable of Knauf not even to mention the preceding epigraphical fact that is staring him in the face and to justify his reading which is another proof, as his "s^{3}," that he reads what he wants to read instead of what the worker did engrave. It was not a question of space: Knauf spends four lines of his commentary on his "ms^{3}" (pp. 46-47) and the bottom 18 cm of p. 47 are blank.

8 - My reading of the Ḥaḍr seal Ja 3204 = InPAS based on the published documentation is as follows:

obverse	↑ qfn	1	Qifan of [the family]
	↓ mḥm	2	Muḥiam,
	↓ qḍw	3	has ratified
reverse	↑ qy ʿwʾ	4	the powers of ʿAwaʾ.

L. 1: qfn, cf. Saf, e.g. Safar 3; cf. Ar qafana "percussit baculo vel scutia," and qifan "durus, inmitis" (cf. FrLAL, III, p. 482A). SaAIC refers qfnt (p. 485) to WH "430a." This text does not exist in WiHaI (p. 112); the text referred to is WH 429a, where the editors erroneously read "qflt" instead of qfnt.

L. 2: mḥm: Knauf does not refer to any parallel. According to Knauf, the name derives "from the root HMY or, less likely, from the root HMM" (p. 46). The apparent reason for his choice is that that etymology is commonly accepted. But, such a reason is welcomed only by sheep-like persons. Against the etymology of mḥm from the root ḥmy is that there is no reason for looking for an exception. Furthermore, according to Knauf, mḥm would be an hapax legomenon since he refers only to Thamudic mḥmy of Jsaf 699, which he erroneously labels "South-Safaitic." The name mḥm is also attested in Saf CIS 1359; cf. JaSN, p. 57, note 62, where the name is related to muḥâmm (from the root ḥmm) "constant, firm, steady;" cf. also the Sab proper name mḥmtn of RES 4176/11.

L. 3: qḍw, cf. Ar qaḍā (i) "decrevit, sanxit, determinavit, praedestinavit" (cf. FrLAL, III, p. 460B). Cf. below, Appendix II on Ja 616/4.

L. 4: qy, cf. Ar quwwat, pl. qaw^{aa}/qiw^{aa} "power, potentia" (cf. FrLAL, III, p. 519B); here, in the moral sense. - ʿwʾ, cf. Ar ʿawā' "defectus in initio, nunquam initiens" (cf. FrLAL, l.c., p. 247B).

The Ḥaḍr seal Ja 3204 contains a warrant of attorney granted to ʿAwaʾ by his Ḥaḍr employer Qifan who, having settled in the North, borrowed some northern designs to illustrate his seal. Such a delegation of power is at home on a seal which is to be used by Qifan's representative anywhere he goes on business for his master. It is quite interesting to note that the inscription of this new seal is in the line of that of RES 3515A = the Beit stamp and its identical, yet different, Bethel stamp, viz. "... [Ṣa]miyān, the delegate" (cf. SASOS, No. 151, October, 1958, pp. 11-14). For more on the Bethel stamp, cf. above, VII.

The question of the dating of the seal Ja 3204 must heed the Ḥaḍr information. Considering that the peculiar form of 𐩭 seems to have been the next to the last example of one particular branch of ʾ characterised by angular forms and that that branch did not survive very long after the last flourish of cursive forms (cf. JaPREH), it would seem reasonable to suggest a date of roughly the 11th-10th centuries B.C. for the seal itself.

D - Appendices.

1 - Lines protruding beyond the normal letter form (cf. above, p. 282).

Lines protracted beyond the normal letter form are attested in several seals or similar antiquities; here follow a few examples.

a - The ʾ of DrHP 1, b of DrHP 3/2 published by F. Bron in AION, 37 (1977), pp. 237-238, and pl. 1a and c, respectively. The author reads the second name of the latter as "bḫm" without suggesting whether he considers it as belonging to a person or a family. More recently, however (cf. Syria, 62 [1985], pp. 339 and 340), he corrected, without saying it, his original reading to "b Ḥḍn," thus, "son of Ḥḍn."

b - Another example of protrusion is given by the scarab published by A. van den Branden in Syria, 35 (1958), pp. 110 (with photo) and 112. Later, F.V. Winnett released a reading different from that of van den Branden in PSAS, 10 (1980), p. 137, and facsimile on p. 139, no. 3. For the original text of Winnett's paper, cf. already JaMAS, XXII, pp. 176-182. But, the reader of the new version of his paper is informed in p. 139, note 1, that "the present revised version, however, includes points on which I have sin-

3

Winnett's facsimile
in PSAS, 10 (1980), p. 139

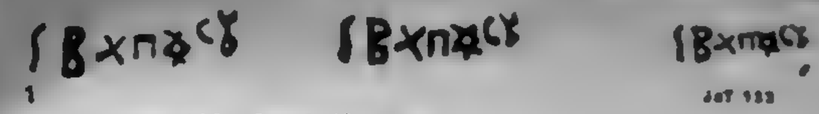
My tracing of JaT 132

ce changed my mind." It is irrational to let stand in a paper "points" which have been rejected instead of expunging them. It is also unfair to the reader not to point them out for the reader has no way of knowing what remains valid and what has been rejected. As far as I am concerned, Winnett's restriction has no value because it is an easy way out of any objection. Indeed, Winnett could always say that a point rejected by someone else is one of the "points on which I have changed my mind." Nobody could prove that he is not telling the truth; neither can he prove that he is right. Furthermore, Winnett's facsimile grossly misrepresents the original and, consequently, his remark on ṣ is erroneous as well as his reading of "k" of his "nʿm bkm Naʿām son of KM" (p. 137); thus, the extreme left letter becomes a wasm! Recently, F. Bron dealt with the same scarab in Syria, 61 (1985), pp. 338 (with a photo and the museum siglum "B.N. 1972. 1317.106") - 339. The author endorses Winnett's reading with the exception that "k" is read "s;" thus, "sm." Neither Winnett nor Bron refers to my study of that last letter; e.g., in JaTS, pp. 56-63; it is, of course, very easy to discard the letter as a wasm. The two photos published by van den Branden and Bron complement each other; e.g., the upper vertical stroke of ʾ is clearer in the former, but the upper horizontal bar of the same letter is visible only in the latter. Yet, Bron makes no palaeographical remark. My facsimile shows the location of the protrusions. The text is republished here as JaT 132 which may be read as follows:

nʿm 1 Naʿam,
b ʾmṣ 2 son of ʾAmaṣṣ. - Cf. hmṣ in Eut 581.

Winnett also deals with three other small inscriptions, his. nos. 1, 2 and 4; they are dealt with below.

c - Winnett's no. 1 (p. 136, and facsimile in p. 139, no. 1) = WiTa 4 (cf. WiReAR, p. 96, facsimile in p. 222, and photo in p. 225). The edition reads the text as follows: "*ʾlmt bīrṣ By Mawt b. ŠRṢ.*"
It should be noted first that Winnett's new facsimile differs considerably from the first, especially in the tracing of the last three letters, viz. there are



1

Winnett's facsimiles in
WiReAR, p. 222 PSAS, 10 (1980), p. 139 My tracing of JaT 133

four protrusions in the first instead of five; the second letter is angular instead of well rounded; and the lower half of the third clearly shows a loop instead of a triangle completely inked in. The latter change was intended to substantiate his reading of the letter as "ṣ."

Winnett's argumentation on the antepenult letter is inexplicable because he did not even see that he contradicts himself; he wrote this (p. 136): " the [A] occurrence of a sign which provides evidence that the Taymanite alphabet contained
[B] the *samekh* (or *s*³). Jamme [1971, 523] believes the sign to be *ḍ*, but elsewhere in
[C] Taymanite the *ḍ* is identical or almost so with South Arabian *ḍ*. It has some resemblance to Thamudic *š* in the form of a circle with projecting rays, but it would be an error in method to attempt to explain the Taymanite sign by one which appears elsewhere only in a type of Thamudic current in the 3rd century A.D. [Winnett 1937, 41,52].
[D] The only alternative remaining is to take it as a *samekh*; certainly it is the South Arabian *samekh* that it resembles most closely."

Ad [A] - Not only is "samekh (or s³)" is ⚹ which is normally called sîn, "samekh" being reserved to ⚹, but this sîn does not exist in NA; therefore, his initial affirmation is the mere statement of his thesis which is yet to be proven.

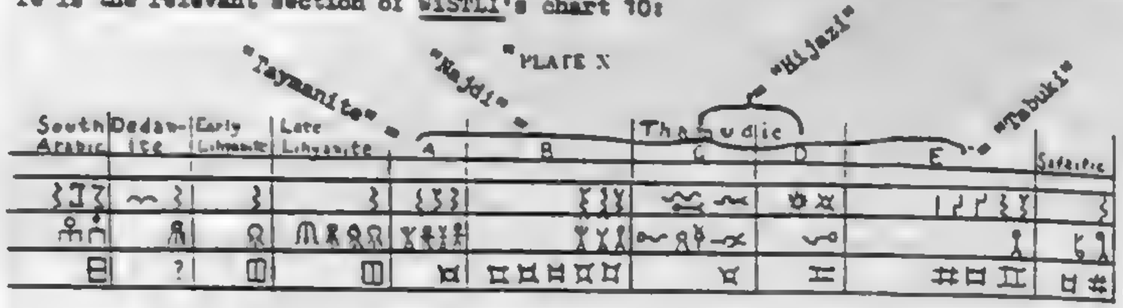
Ad [B] - Winnett's objection against my reading is invalid because he cannot prove that the use of a different letter type is impossible in the Tayma area.

As a matter of fact, his own reading of the antepenult and last letters contradicts his own statement.

The three š of WiTa 5, 20 and 39 have all the ⚹ form composed of five sections, but he reproduces them in his chart (p. 205) as ⚹, thus with six sections!
His reading of the last letter as "ṣ" also contradicts his own statement because that type of ṣ is not found in either his A of WiSTLI's pl. 10 (cf. below) or his "Taymanite" of WiReAR's pl. 1 of p. 215. WiReAR's collection of "Taymanite" texts shows the following types of ṣ, viz. the four most commonly used ⚹, ⚹, ⚹, ⚹, but also ⚹ (no. 38) and ⚹ (no. 47).
For the justification of my reading, cf. immediately below.

Ad [C] – What he is referring to is WiSTLI's pp. 41 and 52, which deal with hi[s] class "D," which, combined with his "C," was rebaptized "Hijazi" in WiReAR's p. 205. Such a comparison remains unjustified because the information given in WiSTLI's chart 10 shows that the present letter is definitely closer to ḏ of his own "A" = the "Taymanite" of WiReAR's p. 205, than to ḍ of his "D." Here is the relevant section of WiSTLI's chart 10:

"PLATE X"



In the normal letter form of ḍ in his "D," the rays are long and much longer than those of the present letter, and also at least six in number. Contrarily, a circle or another geometrical form with four short rays is ḏ, and as such the letter is represented in all his classes of Tham from his "A" to his "E," inclusively. Therefore, by referring only to his "D," Winnett voluntarily eliminates the major part of his own information and creates an opposition which does not exist, but which he wanted to build up for the sake of his cause. Therefore, my reading of that letter as ḏ is plainly justified.

Ad [D] – This is a conclusion based on a fallacious, contradictory argumentation.

The last element of Winnett's thesis is his explanation for reading as "ḍ" a sign which he declares to be "ṡ." For that explanation, the reader has to go back to WiReAR (p. 96, commentary of the edition of the text) where he finds this:

> "it is probable that the *ṡ* sign came to be used in Taymanite as an alternative for the normal *ḍ* sign (see also JS 454), as happened in Hijazi Thamudic (see Pl. 1)."

This statement, which does not prove anything, is a mere rehash of his thesis presented now as a "probable" opinion. But, where is the epigraphical proof that ṡ should be an "alternative" of ḍ? On what basis does he inject JsaT 454 in the case? The form of the last letter is exactly that of Winnett's "A" = his "Taymanite," where it is given the value of ḏ! Finally, why does the author involve his "Hijazi" = "D" since he himself rejected as "an error in method to attempt to explain his 'A' by his "D"? Cf. above, p. 291, the second half of his [C].

As to the letter of JaT 130, the large speck to the right of the center of the vertical staff is as relevant to the letter as the other speck found immediately below which, of course, is not accounted for in Winnett's facsimile. Note also that it is not even appended at the bottom of the staff, as it should be, if it were a part of the letter.

E.A. Knauf incorporates his study of the text in his "Supplementa Ismaelitica, II" in Biblische Notizen, 21 (1983), pp. 39–41, with his facsimile in p. 40: "Abb.1: WiTay 4"

1 – After reproducing WiReAR's reading of the text, he first mentions that the last letter is "eher ein h" (p. 40). If the reader does not read carefully the fine print of footnote 18 (p. 40), where the information is sandwiched, he would never know that Knauf finally acknowledges that the reading is not his, but mine. Furthermore, his reading apparently is based on "einer Beschädigung am Schaft" (p. 40). His position is untrue on two accounts. There is no "Beschädigung" of the letter staff; as stated above, the speck on the right of the letter is adventitious and does not concern the letter. The true reason for endorsing my reading is that his reading of the penult letter as "t" would give an impossible name, viz. "trḥ," if he accepts Winnett's reading of the last letter. He had no choice but to get rid of that "ḥ" and to accept my reading of h.

2 – He, then, claims that the antepenult letter is "eher als ein ṯ" (Abb. 2:1) ein t" and "genauer: das missing link" (p. 40) between Liḥ and Tham t. What he means is that the Liḥ and Tham forms would be independent, parallel derivations (!!!) from that letter form. But, a simple look at the facsimile shows that the latter distorts the form of every single letter, and his fig. 2 amply demonstrates that he missed the mark: the main characteristic of all Liḥ t is the length of the bars added to the central body which even disappears in the late Liḥ texts. On the contrary, the main characteristic of the present letter is a large central circle-like form with four tiny linear additions; and this form is basically the ḏ letter form. He also sides with Winnett in his criticism of my reading of that letter as ḏ by referring to PSAS, 10 (1980), p. 136. Here again, Knauf did not look very closely at Winnett's arguments which are dealt with above, pp. 291–292. – On his reading "trḥ" (p. 40), Knauf refers in footnote 19 (p. 40) to "<Taurat mit dem Übergang -t> -ḥ." This is a preposterous pan-Ar theory which is not even new, as if the qtlh formation would derive from the qtlt formation. In an attempt at proving his theory, Knauf refers to malah of Liḥ JsaL

[illegible], ḥmyh of Stiehl 28 (=10)/1, and ʿydh of Stiehl 13 (= 11)/1 (cf. footnote 19 in pp. 40-41). In all those names, the same question must be raised: what proof, except his pan-ar position, does Knauf have to substantiate his "Übergang"? He does not give any; he never gave any, except a mere affirmation. Why could the -h ending not refer to the god dġbt? Furthermore, there are several other names in the Stiehl collection belonging to the qtlh derivation; e.g., kbrh of Stiehl 18 (= 4)/1-2, ṣrmh of Stiehl 21 (= 6)/1, and especially "nṣrh" (to be corrected to mṣrh) in l. 2 of the text which has ḥmyh in l. 1. Why are they not included by at least an "usw.;" viz. why to select only those two among them? Moreover, why does Knauf have to go to Liḥ in search of examples when several Tham names are more relevant than his Liḥ examples since the two nominal forms exist? Cf. e.g., ʾmlh/ʾmlt, ddh/ddt and dnh/dnt. Finally, Knauf would have to explain the two different forms of t in the same text, such as JsaT 454 reproduced above (p. 292).

The text, JaT 133, may be read as follows:

lmt b ḍrh By Matt, son of Ḍarruh.

mt, e.g., JsaT 55+56. - ḍrh, cf. already in JAOS, 92 (1972), p. 523B.

d - Winnett's no. 2 (p. 136, and facsimile in p. 139) = RÉS 2688.

E. Osiander, in ZDMG, 19 (1865), p. 35 f

Winnett

RÉS 2688

My tracing

Winnett presents his new interpretation as follows (p. 136):

"One of these is the gem in the Hofmuseum at Vienna[6], with the inscription ʿlyh bśg'dhd "Elijah son of Śaggī'dād" (Fig 2). The use of b for "son" suggests that the object is of Taymanite origin, though its owner was manifestly an Israelite. This classification is supported by the type of script employed."

Winnett does not identify the gem as being RÉS 2688. His endnote 6 (p. 139) begins with this information: "6 Müller 1889, 20 and Pl V; also RES 2696. The name ŚG'DHD is Aramaic (śaggī' "great, exalted")."

The first abbreviation is not listed in p. 140. His "also RES 2696" is his no. 4 which he studies in pp. 138-139; but these pages never identify the antiquity as RÉS 2696. Furthermore, his dissection of the text (cf. already WiSTLI, p. 50, and endorsed by vdBrID, p. 33) remains unjustified because the existence of the Hebrew name ʾlyh cannot prove that it must be read here; therefore, his [illegible] tion that "its owner was manifestly an Israelite" remains "manifestly" unjustified. Moreover, his vocalization of the second name which considers "h" as a mater lectionis, was also suggested by W.F. Albright, who stated this: "[illegible] DHAD (i.e., ʾElyahab Sagiʾdad)" (cf. G. Ebeling, Geschichte und Altes Testament, Tübingen, 1953, p. 3). But, each of the two names is taken as "un nom juif" by vdBrID (pp. 34 and 45), who specifies that "le sens araméen [of the first element of the second name] est à exclure" (p. 44) and "dhd" is "un simple emprunt [by] l'auteur juif" (p. 45). However, since the letter forms are neither Aramaic nor Jewish, how is the reader supposed to understand that a non-Israelite would have taken as his personal name not only an ordinary Jewish name but a theophoric one? Furthermore, were the name to be read ʾlyh, it would be a qtyl formation from the well-known root ʾlh. Moreover, Winnett's classification in "Taymanite" is NOT "supported" by "the type of script employed" because the lettering points directly to the Min and Liḥ characteristics of the texts from al-ʿUlā; viz.

for the general aspect, cf., e.g., Ja 2259, 2260a, 2277;

for the type of g, cf., e.g., Liḥ JaL 63d, e, 74c;

for the type of y with a very large circle or another geometrical design, cf. Liḥ JaL 61a, 63f and 71; and

for the squarish type of d, cf. Ja 2336a, 2338/2, 3, 5 and JaL 63a.

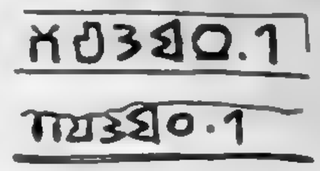
As for his unexplained, unexplainable hypothesis of h as a mater lectionis, WiSTLI (pp. 49-50, and facsimile in pl. 8, no. 2) seeks a proof in his reference to a scarab published by M. Cohen, Les documents sudarabiques, Paris, 134, p. 51 and pl. 15, no. 34; cf. my tracing on the right: Without any analysis of the name śgʾdd, Winnett declares it to be "Dedanite" because of the Aramaic adjective śaggīʾ mentioned above and, without any interpretation or translation (cf. endnote 6, p. 139). But, WiSTLI had stated the name to be "clearly sh-j-'-d-d, "Great is Adad", or a n.pr. Shajīʾadad." (p. 50). For vdBrID, the name "Šagiʾdōd" is "dédanite" (pp. 19, 69, no. 67, and pl. 4, no. 67) and means "Dōd cause de la joie" and "Dōd est une divinité cananéenne" (p. 24). Finally, Knauf states in Biblische Notizen, 21 (1983), p. 39, footnote 16: "Gemein altnordarabisch erscheint der Name, etymologisch korrekt, mit sg: śgʾdd bei A. van den BRANDEN, Les inscriptions dédanites (Beirut 1962), pl. IV:67."

The name śgʾdd of the seal may be considered SA because of the element dd which frequently occurs in SA; cf., e.g., ddwel, ddkrb, ddnmr, ʿlydd, etc. The nominal derivation qtʾ from a root with final w/y is also attested in śrʾhmw of

Ja 401/2. The Liḥ influence is visible in the form of the second letter. The name may be read as follows: Šagi'dawd. Cf. Ar šajiya "to be choked" and šajw "anxiety" (cf. LaAEL, p. 1510A and B, respectively).

Then, to complete the confusion, Winnett compares the preceding text with Eut 681 = Hu 495, which he misreads as " l-.-'-m-sḫ-j-'. The second sign is of uncertain value;" cf. WiSTLI, p. 50, who claims it to be "Dedanite," as vdBrID (p. 65, no. 53, and facsimile in pl. 3, no. 53). vdBrID reads the text as "l. ʿmšg' - Par ʿAmmšagi" (p. 65) but his facsimile reproduces the second letter as a minute dot in the upper line, a, and eliminates it altogether in the lower line, b. Note also the disappearance of the letter ' in the translation.

On the right is the reproduction of the facsimile published by C. Huber in Journal d'un voyage en Arabie (1883-1884), Paris, 1891, p. 327. The text is plain Tham and may be read as follows: lnʿm fg' By Naʿm. He has cohabited with a woman. Cf. JaMAR, VII, p. 81.



Hu 495

In his aforesaid paper (cf. above, p. 293) of Biblische Notizen, 22 (1983), p. 39, Knauf deals with RÉS 2688 in a short expose of nine lines only (ll. 15-23). It is a real feat to concentrate so many irregularities in such short a space.

a - Knauf's reading of the text is as follows:

" 'lyh b(n) s_3g'dhd "Elia b. Saggī'dād"."

The letter "(n)," which does not exist, must be put between brackets, and Knauf assumes its existence because of the gratuitous preconceived theory that the text contains a pedigree.

b - The first phrase of his commentary contains an error: his "(RES 2696)," which is the text no. 4 studied below, must be corrected to RÉS 2688.

c - The balance of his commentary is most amazing.

On the one hand, he rehashes Winnett's opinion inasmuch as he declares that "Der Verfasser ist offensichtlich ein Jude, sein Vater trug einen aramäischen Namen."

These two statements are gratuitous based as they are on the preconceived division of the letters.

On the other hand, he disagrees with Winnett inasmuch as he states a well-known fact, which was conveniently disregarded by Winnett, viz.

" h als mater lectionis für /ā/ ist im ganzen Altnordarabischen unerhört,"

Yet, his attempt at justifying his contradictions of taking ś as "ʿ" and h as a mater lectionis is nothing but an extraordinary tall yarn, viz.

" von dort dürfte der weltläufige Verfasser auch das Zeichen s_3 für das /s/ seines Vatersnamens bezogen haben, das es im Altnordarabischen seiner Zeit schon nicht mehr gab[16]."

"von dort" means "from SA." What kind of revelation - and, if yes, from whom? - did Knauf receive to base his hypothesis of a globe-trotting Jewish author for the text? Really, Knauf shrinks from nothing, and "la belle du logis" of his is free to wander anywhere in the limitless domain of fantasy fiction in a wild effort to justify his concoctions, viz.

a - a so-called Jewish author would have abandoned his own alphabet in favor of a mixture of "dedanisch oder taimā-thamudisch" and of SA ś;

b - he would have engraved a text which is not "taimā-thamudisch" but "Altnordarabisch;"

c - he would have introduced in the text two items which do NOT exist in NA, viz. the letter ś and the mater lectionis, because, being a "weltläufige" person, he borrowed them from South-Arabia!!!

In his aforesaid (p. 289) paper of Syria, 62 (1985), p. 341, F. Bron endorses Winnett's opinions with a few remarks. It is first of all regrettable that Bron fails to seek the siglum of the Cohen 34 scarab at the "Cabinet des Médailles" and to publish it. Bron's first remark is that "il semble plus satisfaisant de lire 'lyh S_3g'dhd, avec les deux noms juxtapposés." He unfortunately does not justify why his reading is or would be or should be "plus satisfaisant;" he does not register the fifth letter, b; nor does he justify the absence of bn son of in RÉS 2688 since he suggests the name of Cohen 34 to be that of the father of the author of RÉS 2688. Finally, he does not volunteer the reference to any parallel of his understanding of the latter. - Bron's second remark concerns Winnett's hypothesis of h as a mater lectionis; and for him, it is a "problème"! In fact, such a suggestion is simply unacceptable and Winnett neither proves nor even attempts to prove it. - Bron's third remark is most obvious, viz. "pourquoi ces deux Araméens ont-ils eu recours à cette écriture?" The real question, however, actually is: Why to keep speaking of Aramaeans when the script of the text is not even Aramaic? Finally, he relegates to footnote 27 E.A. Knauf's suggestion of a "weltläufige Verfasser" without any comment or remark!

The text of RÉS 2688 may be considered SA and the nature of the antiquity suggests the following reading:

ʾlyhb sgʾ dhd ʾIlyahab, trusted friend of Dahad.

ʾlyhb, cf. ʾlwhb, e.g., in Gl 1256/1 from al-ʿUlâ. – sgʾ, cf. Ar sajîy "socius et amicus sincerus" (cf. FrLAL, II, p. 288B). – dhd, cf. Ar dahdá "enrouler, rouler" (cf. LaGD, I, p. 859). The name dhd is that of the man represented on the seal.

e – Winnett's no. 4 (pp. 138-139 with facsimile, no. 4) = RÉS 2696 = BM 89155. Winnett's facsimile is on the right.

Fascinated by his "Taymanite" and as it normally happened in his publications, Winnett does not at all care of previous published papers unless they are written by his friends who favor his ideas or close their eyes on his irrational approach to research. He gives another example of such unscientific a method by not even mentioning that the cylinder involved was published as RÉS 2696 (cf. above, p. 294) and that its present siglum is BM 89155, as it is stated in JaMAR, II, p. 384. Consequently, he does not even refer to my study of that antiquity in JaMAR, l.c., pp. 56-57 and pl. 4, which is reproduced here.

4

"RÉS 2696. – Cf. tracé à la pl. 4. – Pour un résumé des différentes interprétations du texte gravé sur ce petit cachet qui fut publié en 1863, cf. A. van den Branden, dans Les inscriptions dédanites, Beyrouth, 1962, p. 29-32, où l'auteur expose sa propre interprétation, dont la discussion est donnée ci-dessous.

a – L'auteur affirme que les deux lignes sont sénestrogyres, mais ne donne aucune raison. La direction des lettres de bn "fils de" prouve la direction sénestrogyre de la l.1 qui, autrement, restait douteuse: d et r sont sénestrogyres, k et n sont dextrogyres, et les deux b peuvent être lus dans les deux directions. La direction sénestrogyre de la ligne inférieure est donnée par la position de r; les trois autres lettres sont dans la même condition que celle des deux b de la ligne supérieure.

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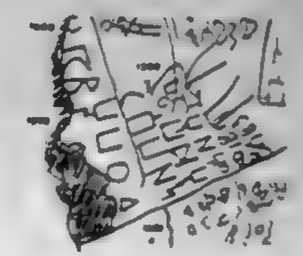
Je propose de lire le texte comme suit:

dbrk:bn	1	Dābiruk, fils de
yryɔ	2	Yurayyiɔ.

dbrk: pour la première partie du nom, cf. l'ar [illegible] "[illegible]", [illegible]; le [illegible] est le pronom personnel de la 2e personne du singulier, qui se rapporte à la divinité. Le sud-arabe dbr (e.g., RÉS 3285/5) signifie "travail, travail fourni". – yryɔ: la lettre triangulaire n'est pas d, qui existe au début de la l.1 avec la hampe verticale. Il s'agit de y comme dans les graffites qatabanites de la région de Mukérâs (p.ex., Ja 1598/1, à paraître prochainement), où l'on trouve aussi de nombreuses formes de y avec une appendice minuscule. Cf. l'ar [illegible] (o), 2e forme "réfléchir sur une chose".



RÉS 2696"

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ɔbʿy	←	1	ɔAbʿay
rhb dn	←	2	[of the family] Rāhib, he of (the group) Nā-
ɔd b	→	3	ɔib, in
ɔdn/ʿ	←	4	submission to ʿAmm
m/w	←	5	and
šmsm	←	6	Šamsum.

The present text was engraved after the following inscription. – ɔbʿy, cf. the Saf personal name bʿy (e.g. CIS 617). The letter y is a tailless triangle; cf. the commentary on RÉS 2696 (cf. JaMAR, II, pp. 56-57). – rhb, Sab CIH 556/1. – nɔd is a Min personal name in Gl 1284/1."

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"[9] In Dedanite bn is always employed for "son". JS 381 might seem to be an exception but this inscription, though found in the Dedanite area, is really Taymanite as the form of the d shows. The b in JS 124 may well be for bū = ʾabū."

JsaL 381 is plain Min (cf. JaMIL, p. 54); even vdBrID does not include this text among his "Dédanite." In JsaL 124 Winnett's suggestion that b "may well be for bū = ʾabū" is a desperate, but completely baseless concoction presented for the

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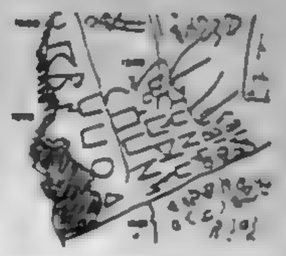

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sake of the cause and of which he cannot give a single example. In the text, which he also considers as "Dedanite" (pp. 67-8, no. 61), vdBrID joins b to ʿly to read "bʿly mon maître," thus inventing another unproven concoction also presented for the sake of the cause. The preceding example is clear: the two authors a priori decided which text is "Dedanite" and then either they exclude the others which do not fit their preconceived criteria or they invent rules which make it possible for them to maintain their initial preconceived choice. For JsaL 124 = JaL 71, cf. JaMIL, p. 63.

3 - Ever since JaMIL, I rejected the terminology of "Dedanite" because those so-called "Dedanite" inscriptions actually are either Lih or Min. As expected, Winnett rejects such a proposal saying that

> "The acceptance of these ideas would, in the present writer's opinion, throw ancient North Arabian epigraphy into complete chaos" (p. 139, endnote 10).

If the reader wishes to have a good look at "a complete chaos," he may find it in Winnett's stand and ideas presented in the preceding pages or in the historical sketch of the so-called 'Dedanite" inscriptions offered by the aforesaid publication, pp. 2-10. That such chaotic a presentation is an exception instead of a way of life for Winnett is disproved by his latest paper printed in JAOS, 117 (1987), pp. 239-244, as amply demonstrated by my discussion of that paper in JaMAR, XV, pp. 124-191. For "Dedanite," cf. already above, p. 112.

4 - Winnett states that "this triangular sign, which appears twice [in l. 2], is a problem" (p. 138) and gives another example of unscientific argumenting method when he claims the reading of the letter as "d" "plausible" because of the "Syriac dardā' 'vulture';" and he reads the word as "drd'." The letter d is clear in l. 1 and above (p. 298), my commentary on RÉS 2696. Moreover, here again, A.F.L. Beeston could not resist the temptation of stepping in to give yet another example of his imaginative extravaganza. According to Winnett,

> "Beeston has remarked that if the triangle is a *d*, then the triangle plus upright which is the first letter of the text can hardly be the same letter, and that one might have to envisage the form with the upright as an early form of *d*."

5 - Winnett does not make any remark on the initial name dbrk. Therefore, the reader is authorized to think that he still maintains WiSLTI's commentary (p. 49) which reads as follows:

> "It seems preferable to explain the first name from *d-b-r-k*, "club, mall" (cp. Dozy, *Supplément*...)."

Dozy copied the word from the Egyptian E. Bochtor's Dictionnaire français-arabe, Paris, 3rd edition, 1864. In the 2nd edition, Paris, 1848, the Ar word is under "mail" and "massue" in pp. 475A and 489B, respectively. The word was not utilized in my study of dbrk because it is neither found in any other dictionary known as a derivation of brq. I am grateful to Larry B. Miller for the following of last August 8, 1988, which reads as follows:

"مبرك (عند بختور وبوزنه) والصواب: مَبْرَس"

In his Supplément (vol. I, p. 423a9), Dozy cites the word dbrk with the meaning "mail, massue" on the authority of Bocthor's Dictionnaire français-arabe 3d. ed. (Paris, 1864). The word is not found in any other source.

It appears to me that Bocthor or his source misread the Arabic word written dbws, vocalized dabbūs and dubbūs, which means "a mace of iron or other material" (Lane, Lexicon III 849a3). The word apparently comes from the Persian dabūs, not dabuz as reported by Lane, and means "a club or iron-headed mace used by cavalry" (Steingass, Dictionary 503b). It seems that the letter waw was mistaken for a rāʾ and that the letter sīn was mistaken for a kāf, resulting in the mistaken reading "dabrak."

Larry B. Miller
Nach Dictionary Project"

2 - qtḍn of Ja 616/14.

The saga of Sab qtḍn, which is presented here in connection with qdw of l. 4 of Ja 3204, is yet another example of speculative concoction, which has become the trademark of the "laudemus invicem" club members.

The context is as follows (cf. JaSIMB, p. 114):

" 12	— — /lib^{2}/[	/w,wafym/f^{1}b/w^{1}tr/ḫwln/gddm/wlhm
13	[dw]/ḥ,y/[wmqnm/w^{1}hm^{1}	ʿhm,w,f^{1}lmqḥtmnb^{1}f^{1}mm/bḫt/t^{2}
14	[t]mw/wqtḍn/ki/f^{1}b/w^{1}tr^{1}	ʿtr/ḫwln/gddm/ — — "

JaSIMB translates the pericope as follows (p. 115A):

> " 12 — — to fight and to protect the tribes and groups of Ḥawlān Gaddum; and [they have] praus[ed]
> 13 the strength and the power of their lord ʾIlumquh Taḥwān, master of ʾAwwām, because have
> 14 joi[n]ed and settled down all the tribes and groups of Ḥawlān Gaddum — — "

The preceding text is paraphrased as follows (cf. l.c., p. 340A-B):

> "These two are assigned the duty (lines 9-17) of protecting and fixing the groups and tribes of Ḥawlān Gaddum."

a - Beeston was the first to deal with qtḍn; he writes this (cf. Le Muséon, 85 [1972], p. 343):

> "This seems incongruous with the preceding passage, where we are told that the dedicators were given the task of 'to fight and to protect' these tribes.

I suggest that the verb is a consecutive infinitive terminating in n from a weak root, e.g. Ar. *iqtadā* «require, demand»: the Ḫawlān «assembled (*t'tmw*) and were called up (for military duty)».

Beeston's opinion is endorsed by J. Biella, Dictionary of Old South Arabic Sabaean Dialect, Chico, 1982, p. 465.

1 – How can an army "fight" and "protect" the very same persons at the very same time? Beeston's obscurantist attitude forced him and his henchman J. Ryckmans (cf. below), to all kinds of errors. He refuses to understand what is obvious, viz. that the expression lsb³/wwfyn means "to fight in order to protect;" and, therefore, he erroneously considers Ḫawlum Gaddum as the enemy.

2 – His interpretation of qtḏn as related to the root qḏw/qḏy is contradicted the infinitive wfyn which testifies that the third radical should not be assumed to be elided without a valid reason. But, Beeston does not give any.

3 – He takes as synonymous the verbs "require, demand" and "call up."

4 – In order to give some meaning to his translation, he gratuitously adds "for military duty," an expression which has no counterpart in the text and, therefore, should have been put between brackets since it is the result of a commentary that brings into the text an idea foreign to the inscription.

b – Two years later, J. Ryckmans added his grain of salt to the case as follows (cf. Le Muséon, 87 [1974], p. 248, footnote 2): "Nous rapprochons de l'arabe *qāḍa*, «défaire, démanteler», qui signifie à la 5e forme «mettre en déroute», et à la 7e: «être mis en déroute».

This is the type of remarks which Ryckmans is famous for and uses from end to end in his papers.

1 – Ryckmans sheepishly endorses Beeston's error.

2 – Ryckmans does not specify whether his qāḍa is a verb with medial w and y, and using only A. De Biberstein Kasimirski's Dictionnaire arabe-français, he adapts it to his own whim. Vol. II of this dictionary (Paris, 1860), pp. 834B–835A, has this:

" قَاضَ *f. O.* Défaire, séparer les parties qui tenaient ensemble (*p. ex.* une construction en bois, en ôtant les pièces qui la composaient); *fig.* mettre en désordre. II. (*n. d'act.* تَقْوِيضٌ) 1. *Voy. la* I. 2. Abattre, renverser, démolir, *p. ex.* une tente en arrachant les pieux et en ôtant les cordes qui la maintenaient. V. تَقَيَّضَ 1. Être mis en désarroi ou en déroute; enfoncer (un escadron de cavalerie); mettre en désarroi, détraquer (une construction). 2. S'écrouler, tomber tout à fait (se dit d'un

[illegible] 3. Aller [illegible] qui se répand de côté et d'autre. VII. 1. Être détraqué, disjoint et mis en désarroi (se dit d'une construction). 2. Être enfon- [illegible]

Ryckmans' "défaire" is copied from the dictionary's 1st form;
"démanteler" summarizes the dictionary's 2nd form;
"mettre en déroute" is an adaptation of "être mis en déroute" of the 5th form; and
"être mis en déroute" is copied from the dictionary's 5th and 7th forms.

But, no active meaning of qāḍa (o), 5th form, is given by FrLAL, III, p. 515B = LaAEL, p. 2575B, etc, where the first meanings of the 5th form are, respectively, "dissoluta fuit compages, acies; Dirutus fuit" and "became demolished, destroyed, or thrown down." The active meaning of the 5th form is unjustifiable and, therefore, unacceptable.

3 – After reading Ryckmans' comment, the reader still does not know how to translate tqḏn because it is an 8th form, and that form is not listed. And even if Ryckmans considers that there is no difference between the 1st, 5th, 7th and 8th forms!, as his footnote may suggest, the reader still does not know whether Ryckmans gives the 8th form qtḏn an active or a passive meaning. This is another example of Ryckmans' argumenting method: he relishes confusing statements or situations so much that the reader is forced to think that it is his second nature.

4 – Ryckmans could easily have avoided such a confusion had he looked at his favored dictionary, viz. p. 846B, where he would have seen that qāḍa (i) indeed has an 8th form with the meaning of "arracher avec la racine, extirper" = FrLAL, III, p. 522B and LaAEL, p. 2579B: "He extirpated it; destroyed it utterly." Surely Ryckmans could have twisted this meaning to fit his thesis.

c – Beeston again.

Two years after Ryckmans' mockery, Beeston released what may be considered the quintessence of his scholarship, viz. his translation of Ja 616/12-14, which reads as follows in his Warfare in Ancient South Arabia (2nd.-3rd. centuries A.D. (London, 1976). His translation reads as follows:

"12 | to go on campaign and make contact with the communes and nomad tribes of Greater Khawlān.

13 | The dedicants praise the might and power of the

14 deity because / in their military services they reduced to submission the communes and nomad tribes of
15 Greater Khawlān" (p. 29).

The asterisk is explained on the following page (p. 30) as follows:

"* conjecturing t'[d]mw."

Such a translation is nothing but an abject travesty of the original text:

1 - the verb wfy is translated as "to make contact;" fortunately, SDDS does not list such an aberration under wfy (p. 158);

2 - ʾaʿb (twice) is translated as "commune" (cf. also his SDDS, p. 130);

3 - ʿaʿr (twice) is translated as "nomad tribe" (cf. also his SDDS, p. 21);

4 - hwln/gddn (twice) is translated as "Greater Khawlān;"

5 - "conjecturing t³[d]mw" remains unexplained and unjustified; this "conjecture" is not mentioned in SDDS, p. 2;

6 - his plural verbal form "t³[d]mw" is rendered as a temporal complement;

7 - the infinitive verbal form qtḍn is given the value of an independent plural perfect verb. Furthermore, contrary to his 1974 interpretation, where the verb is considered to be in the passive voice, the same verb is now in the active voice, viz. "they reduced into submission;" therefore, he endorses, without saying it, J. Ryckmans' theory and transfers to the 8th form the erroneous meaning of the 5th form.

d - The last stage of the qtḍn saga is in plain evidence in SDDS of 1982, where the messy, political argumenting method of the four authors is evident and confirms once more my appellation of that group as the "laudemus invicem" club. Those authors' twisted contraption reads as follows (p. 110):

"*QWD

v.infl. *qid* E 1385. J 616 14 [M] reduce *enemy* to submission contraindre *l'ennemi* à se soumettre (عدوّ) قهر. (عدوّ) كسر شوكة (JRy Him 3.248 n.2)."

If the idea of the root qwḍ comes from J. Ryckmans, who is referred to explicitely, the suggested translation tells a short story. Since Beeston is the leader of the "laudemus invicem" club, it is natural that his henchmen compensate him for the dropping of his 1972 interpretation (cf. above, pp. 301-302). Therefore, they made their own Beeston's 1976 translation of "to reduce to submission." Thus, they endorsed Ryckman's erroneous reasoning (cf. above, pp. 302-303) and crown their flattery with another contradiction: J. Ryckmans' erroneous "mettre en déroute" does not, and cannot, be synonymous of "to reduce to submission."

The reader unfamiliar to it and to Beeston's obstinate scorn at submitting even as Beeston's and Ryckmans', and keeps asking the question over and over again: How is it possible? The basic reason is that which explains the absence of any etymology in SDDS, where the question is not even mentioned. On behalf of himself and of his three henchmen, Beeston explains that absence as follows in Rayḥān, IV (1978), pp. 24-25:

"It is equally to be noted that the compilers have not attempted an etymological dictionary; ...

... discussion of possible etymological cognates demands far more space than is available in a dictionary."

The preceding question is already denounced in JaMAR, XIV, p. 67, b. The question, however, is so important that it seems necessary to take it up again. Even at first sight, Beeston's statement has a false ring and appears to be a lame excuse or an empty window dressing hurled at the reader's face as a handful of pepper is flung at someone's eyes to blind him. It is quite evident that Beeston must think that his reader is brainless. Instead, it is the kind of statement that people laught at while repeating the local axiom: "Dites-le à un cheval de bois et il vous ruera à la figure." That such is the case is that Beeston himself compares his SDDS with J. Aisteitner's Wörterbuch der ugaritischen Sprache" (pp. 25-26). But, Aistleitner's dictionary does not fail to indicate the Semitic parallels. Who in his right mind expects to find a "discussion ... cognates" in a dictionary? Who in his right mind expects no mention of cognate(s) at all because their discussion would be out of place? In other words, who is his right mind does not know that there is a middleway between those two extremes, which is the solution commonly adopted in dictionaries? SDDS's evident purpose is to prevent the reader from checking their Semitic cognates and, therefore, from having the proof of how distorting their reasoning really is. The preceding case of qtḍn is a good illustration. SDDS gives the 8th form qtḍn the active meaning of the passive meaning of both the 5th and 7th forms of Ar qwḍ, and the active meaning of the 8th form of Ar qyḍ is not suitable. There is no excusing the four "laudemus invicem" club members' stand any more than the complete exclusion of the German language, which was already denounced in JaMAR, l.c., p. 69, as W.W. Müller's total surrender, shameful and insulting to his own culture.

Beeston's and J. Ryckmans' position, however, is perfectly in line with Beeston's statement which reads as follows (cf. JSS, 27 [1982], p. 101):

> "Now the method of arguing from context can of course be (and unfortunately sometimes has been) misused. Yet it remains the *only* possible means of reaching an understanding of a dead language."

This statement is very important because not only the adverb "only" is printed in italics but also its writing is contemporary of that of SDDS and may, thus, be considered as depicting the mentality which presided over the making of SDDS. The following remarks are in order.

1 - In order to prove the accuracy of his statement, Beeston reproduces (cf. bottom of p. 102) a short text written by R.A. Waldron, which expresses such a self-evident and well-known fact of the ordinary life that is a ridiculous truism breaking into an open door.

2 - Beeston's statement is - as he would call it - an "Apologia" for my work. When referring to the fact that such a method " sometimes has been misused," Beeston surely intended to exclude himself although he in fact is a daily misuser of it.

3 - Nobody has ever reasonably denied the necessity of such a method. But, it is a gross error to claim that it is "the only possible means of reaching an understanding of a dead language." What Beeston really had in mind when writing his statement is to justify his own fancy translations, such as those reproduced above, because of the fallacious opinion that those translations would reflect what the context means. In fact, what Beeston normally does is first to decide what the context should mean according to his own whims or even his instinct fed with comparisons taken even from non-Semitic cultures and, secondly, to formulate his own concoctions as his translations. As a good illustration of Beeston's argumenting method, let us summarize here his translation of Qat ʿrn (cf. above, pp. 93-96). His understanding of the context is based on the meaning of an English word coined by medieval European law. Then, he anachronically injects that meaning into Hebrew and Ar, where that meaning does ... of course ... not exist. Finally, he passes it on SA. "Après cela, on peut tirer l'échelle!"

When facing a NA or SA inscription, the basic work is the semantic analysis of the meanings of the involved roots in other Semitic languages, and especially in Ar, because all those languages belong to a specific linguistic group, viz. to gather together from each Semitic language what DAFA does in Ar, viz. to isolate the "Notion de base." The collation of what appears to be the "Notion de

[illegible] cessary basis to start with. Then, but only then, the translator has to adapt the general common denominator to the specific context he is working on with the help of all that is already known by the published inscriptions ... while keeping in mind that the institutions and the way of thinking of all those populations still remain greatly unknown. This fact, in turn, makes it premature and unfounded to be too precise in the wording of the translations and also to accept material parallels even taken from the Semitic world as sure guides.

ONE PAGES b e f o r e Müller's note of JaMAR, XIV, in p. 404. It may be add here that Müller abuses AfO's confidence and columns and, therefore, AfO's reade by listing the publications of his choice only. Here are the numbers of the JaMAR volumes which are NOT mentioned in his "Bibliographie: Südarabien:"

1 - VII (1974) not mentioned in AfO, 25 (1974/1977), pp. 504B and 506A;
2 - VIII (1979)
3 - IX (1979)
4 - X (1979) } .....not mentioned in AfO, 27 (1980), p. 486A, or 28 (1981/1982), p. 432A, or 29-30 (1983/1984), p. 487A;
5 - XI (1980) not mentioned in AfO, 28 (1981/1982), p. 433B, or 29-30 (1983/1984), p. 487A;
6 - XIII (1983) not mentioned in AfO, 31 (1984), p. 339A.

C - "ohne ISBN-Nummer."

Here again, the so-called all-knowing Müller blindly launches an attack like a shortsighted wild boar not seeing the tree ahead which will smash its skull. ISBN does not apply to JaMAR because it applies to periodicals and serials but not to individual monographic series. Further, neither ISBN nor ISSN is binding; the acceptance of either is far from being unanimous and is not carried out uniformly by those who accept them; and many books do not bear either number. One example only: JAOS does not list either number in its "Reviews" and/or "Books received" lists. The situation being what it is, there has never been any reason or even any inclination to request an ISBN number for JaMAR, and Müller's preferences are none of my concern. Furthermore, here again, Müller contradicts himself because his remark is NOT to be found in his listing of any of the five publications listed above, in p. 309. The remark on the last two of them applies here also.

D - "Pamphlet."

Since "Pamphlet" is a loanword in German, the first thing to be done is to make sure of the exact meaning given to the word in German. P. 507C of Duden Fremdwörterbuch. Der Grosse Duden, Bd. 5 (Mannheim, 1966) contains the following entry: "Pam|phlet [*engl.-fr.*] s; -[e]s, -e: bes. politische Streit- u. Schmähschrift, verunglimpfende Flugschrift."

Here again, Müller's preferences on the meaning of "Pamphlet" are none of my concern. If he disagrees with Duden, he should either have published his own dictionary or have added a note specifying the meaning he gives to the word.

[illegible] years ago, I was called a "Partisan," a word meant to be the supreme insult always uttered with a hate and scorn for everyone to see. Now comes Müller who reveals his true face and, once more, voluntarily distorts the truth.

By using a word connected with politics, which have never been and will never be involved in my publications, Müller shows his main concern even when dealing with scientific research, and he does not shrink from projecting the same concern in someone else's work. And, alas, he is not the only one using research to foster his personal ambition.

Of course, Müller lacks the basic honesty of informing the reader of two facts which the latter needs to objectively assess Müller's position, viz.

1 - "several scholars manifested their wish of having a detailed analysis of SDDS in order to be able to assess its true value" (cf. JaMAR, XIV, p. 64). This fact alone indicates that all those scholars had strong reservations on the quality of the work of SDDS's authors.

2 - Each of my statements is backed up by published affirmations or an evident attitude of the authors involved.

There is nothing defamatory or unfair or false or malicious or whatever Müller may like to call it to r e p e a t what they have written or put into words the manifest attitude they have taken and to draw the necessary conclusions from the facts. After all, they have made them public and, therefore, they have nobody but themselves to blame. Instead of accepting the responsibility of his follies and swallowing the pill noiselessly or, if he thinks it to be the case, of explaining the why and how of this or that question, Müller chose the attitude of a child who, after being duly chastised for his prank, snuggles up to his mother's skirts while crying: "He did it to me, Mom!"

One example suffices here. SDDS utilizes in both the 3 titles pages and all through the text three languages, viz. English, French and Arabic; everything is triplicated, viz. English for the British Beeston,
French for the Flemish Ryckmans, and
Arabic for the South-Yemeni Ghul.

But, there is no German for the German Müller although he, too, is a coauthor. This most obvious anomaly, which is not even mentioned by the editors, was denounced on p. 69 of my review as follows:

"Another shocking aspect of the dictionary is Müller's apparent disregard for his own heritage and culture: the German language is excluded from the dictionary as if his Muttersprache was not scientific or civilized enough and, therefore, had to give way to either English or French or Arabic. Such an exclu-

sion is shameful and insulting to his culture no matter how it is presented bec there cannot be any valid reason for such total a surrender. Any reason involving budgetary considerations would already in itself be insulting to the German culture; it would also be wholly unfounded because not only a great number of lines remain unfinished (one more word would not demand another line) but also there is no justification in multiplying the number of translations; e.g., six for ʾdn (p.2) and ʾbr (p.4), and five for ʿbd (p.11) and ʿdr (p.13). Finally, it would be unthinkable, at least for me, that the motion to exclude the German language have been initiated by Müller himself. But, could such a move really be impossible? All that is presently known is Beeston's statement according to which "the four compilers of the work now in progress held a steering meeting in the summer of 1975, at which certain guidelines were laid down for the work. In the first place, it was agreed that the interpretations should be in English, French and Arabic" (cf. Raydan, 1 [1978], pp. 23-24). Except from stating that the responsibility of excluding German is theirs and only theirs, Beeston's statement leaves the reader in the dark on all important questions on that so-called agreement, viz. who suggested that selection of the three languages?; what was the reason for excluding German?; what was Müller's reaction to such a proposal, if any? Any kind of explanation which the authors may give in the future cannot but be considered as a lame pro domo which nobody who is aware of the past, a few examples of which are denounced in the following pages, will take at face value."

I was not the only one to raise the question of the German language. Even G.Garbini asked why the German had been left out in his review in AION, 45 (1985), p. 733; he speaks of the internationality of the dictionary manifested

> " nella sua impostazione trilingue – comprensibile per quanto concerne l'arabo, un po' meno per il francese (perché non il tedesco, allora?), che appare un'inutile ripetizione dell'inglese."

Garbini's argumentation, however, misses the mark. A polyglot dictionary inevitably is nothing but a continuous "ripetizione," and "un'inutile ripetizione" does exist whether the second language is Ar or Russian, whether the third language is French or Italian, etc.

Müller's choice of the word "Pamphlet" is but the result of very strong feelings of resentment that have developed over the years(cf. below) if compared with the closing statement of his listing of JaCMYE in AfO, 26 (1978/1979), p. 396B, viz."Auseinandersetzungen mit Fachkollegen." This expression already manifested not only the toadyish attitude to which the author had committed himself but also his dictatorial belief that the writing of those "Fachkollegen," including himself of course, were ... untouchable and subject only to sanctimonious approval.

E – "der Hauptteil."

Müller who always wants to give the impression of being most precise and most accurate, does not even give his reader any idea of the actual length of that "Hauptteil." It is, of course, to his own advantage to hide from the reader that that "Hauptteil" covers pp. 62-303, thus exactly two hundred and forty-two pages because the reader would then have seen how absurd and foolish is Müller's flattering assessment of "Ausführlich Besprechung" which he gives to A.G. Lundin's review of 6 (six) pages and to H. Preissler's review of 6 1/3 columns in AfO, 33 (1986), pp. 404A and 399B, respectively; thus, exactly on the same page or 5 (five) pages before his listing of JaMAR, XIV, respectively,while A.G. Lundin's first review of the same SBDS and the same author's review of J.C. Biella's dictionary in Le Muséon , 97 (1984), pp. 176-179 (cf. JaMAR, XIV, pp. 64-66 and 173-176, respectively),found no entry in Müller's listings in AfO, 33 (1986),pp. 399A and 402A. Three more remarks on Preissler's review mentioned above. (1) In Müller's text, l. 3, "p." must be corrected to "col." (2) The first 2 1/2 columns are a mere introduction to the 6 1/3 columns of actual review. (3) Last, but not least, these 6 1/3 columns reviews TWO dictionaries, viz. SBDS and Biella's!!! How could such short papers, and especially Preissler's, contain an "ausführlich Besprechung" of two volumes stuffed with nothing but small details? Note also that Preissler's 1979 paper is discussed in JaMAR, XIV, pp. 228-29 and 303.

F – Müller's summary of JaMAR, XIV, is limited to five short quotations copied from the book itself. Here again, Müller distorts the facts and appears incapable of reproducing the texts correctly.

1 – In his first quotation, "Sabaic Dictionary" must be underlined.

2 – In his second quotation, "laudemus invicem" must be put between quotation marks. It seems appropriate to reproduce here a short passage of JaMAR, XIV, p. 68, viz.

> "Incidentally, a few years ago, Müller rather naively made it clear that the expression "laudemus invicem club" was hurting him. Although I most certainly never rejoice at the suffering of any human being – as the members of that club ... quietly rejoice at the destruction of my office,–Müller's confession is the best proof that the expression is plainly justified because of the well-known axiom "il n'y a que la vérité qui blesse." ."

3 – His fifth quotation, viz. "'imaginative and abnormal literary production' (p. 225)," does not exist as such. Müller forged the expression from two distinct phrases, viz.

"Beeston's imaginative literary production" (l. 7) and

"Beeston's abnormal literary production" (l. 48).

4 – Each of the two preceding expressions mentions Beeston's name explicitely. Yet, Müller distorts that crystal-clear evidence by applying it to all authors he lists, "u. a." = "und andere." This is a shameful generalization. F.i., I defy Müller to prove that something similar is said in JaMAR, XIV, of H. Preissler (cf. above).

5 – The name of M. Ghul is conspicuously missing from Müller's list of the "laudemus invicem" club members. It is also shameful that A. Ghul be so soon forgotten by Müller who owes him so much. But, what chance did Ghul have to be remembered by Müller when the latter is seen ... forgetting his own culture?

6 – "ein Rundumschlag."

What did Müller expect? If and when he needs approval, flattery, or anything of the kind, he knows where to get it. Furthermore, SDDS being coauthored by four persons who see only to those who flatter them or to whom they wish to flatter because of some benefit to get from them, any objective, detailed review has to deal with all and each of them inasmuch as possible, and the four coauthors rightly have precedence of the others, and their particular work in SDDS must be presented and understood in the light of their previous publications. Summarily, with Müller and the like, you are "doomed if you do and doomed if you don't." To avoid the doom of any of them, an author simply has to follow him like a sheep and blindly bleat in smug extasy before him or any of his opinions which is supposed to be received as sacrosanct and untouchable as any Delphic oracle, viz. any of them actually practices intellectual slavery through "ipsedixitism."

The reader may judge by himself the quality of information he is fed by Müller through the columns of a periodical which deserves much better. Furthermore, Müller's note does not alter the f a c t that JaMAR, XIV, provided the scholars who wanted my review with an information more abundant and more diversified than they had anticipated, and they appreciate it.

AM 758: pp. 4, 7, 13
CaTh 10/2-3: p. 9
CIH 543 = Ja 3202
559: p. 23
CIS 2958: p. 203
3168: p. 188
ClAS 828: p. 121
ClSTI 5: p. 203
CoNS/2: p. 91 Cohen 34:p.295
Gl A 452/6: p. 92
Gr 27: p. 58
Ḥâ'il 160: p. 124
161: p. 125
203d: p. 120
HaLi 55+56: p. 207
156: p. 222
160: p. 188
351: p. 122
402: p. 125
481: p. 194
490+489: p. 273
Hu 495: p. 296
HulR 134: p. 110
Ja 474/1-2: p. 48
616/14: p. 301
631/1 : p. 48
1093: p. 107
3198 = NAM 224: p. 40
3199: p. 68
3200: p. 81
3201: p. 92
3202 = CIH 543: p. 103
3203: p. 128
3204: p. 251
3205 = RoDAA 4: p. 52
JaS 196: p. 131
JaS 197: p. 131
198: p. 131
JaT 97: p. 109
98: p. 109
99: p. 111
100: p. 111
101: p. 175
102 a,b: p. 176
103 a-i: p. 177
104 a-d: p. 178
105 a-f: p. 196
106 a-z: p. 197
107: p. 210
108: p. 211
109: p. 212
110: p. 215
111: p. 215
112 a,b: p. 230
113: p. 231
114: p. 231
115 a-d: p. 231
116: p. 232
117: p. 232
118 a-c: p. 232
119: p. 233
120 a,b: p. 233
121 a-c: p. 233
122: p. 233
123: p. 234
124 a-c: p. 234
125 a-f: p. 235
126 a-d: p. 235
127: p. 162
128: p. 200
129: p. 200
130: p. 200
JaT 131a , b, c: p.251
132: p. 289
133: p. 290
JoDD 1-7: p. 164
JsaL 138: p. 127
JsaT 248: p. 192
251b: p. 126
NAM 188: p. 12
224 = Ja 3198
483: p. 31
2340: p. 12
2694/2: p. 41
2753: p. 21
Ramm 24: p. 222
RÉS 2640: p. 5
2687: p. 7
2688: p. 294
2693/2-3: p. 6
2696: p. 298
3902, no. 120 A,B: p. 25
3965: pp. 14, 17
4708 A,B: p. 60
4782: p. 90
4829: p. 21
4839: p. 2
RoDAA 4 = Ja 3205
Stiehl 28/2: p. 294
WH 349: p. 224
429a: p. 288
883: p. 204
1768: p. 128
Wi 653: p. 223
955: p. 204
WiTh 64: p. 247
YM 350: p. 72

Major Abbreviations

AlAAE : F.P. Albright, The American Archaeological Expedition in Dhofar, Oman, 1952-1953, Washington, 1982. - Ar : Arabian, Arabic, Arabism.

BeDGSA : A.F.L. Beeston, A Descriptive Grammar of Epigraphic South Arabian, London, 1962. - BeSA : T. and Mrs. Bent, Southern Arabia, London, 100. - BeSG : A.F. L. Beeston, Sabaic Grammar, Manchester, 1984. - BMGC : British Museum General Catalogue of Printed Books, London. - vdBrID : A. van den Branden, Les inscriptions dédanites, Beyrouth, 1962. - vdBrIT : A. van den Branden, Les inscriptions thamoudéennes, Louvain, 1950. - BuWG : F. Buhl, Wilhelm Gesenius' hebräisches und aramäisches Handwörterbuch über das Alte Testament, Berlin, 1962.

CaLL : W. Caskel, Lihyan und Lihyanisch, Köln, 1954. - CIASA : Corpus des inscriptions et antiquités sud-arabes, Louvain, I (1977) and II (1986). - CIH : Corpus inscriptionum semiticarum. Pars Quarta: Inscriptiones himyariticas et sabaeas continens, 3 vol., Paris, 1889-1930; Plates: 3 vol., 1889-1932. - CIS : Corpus ... Pars Quinta: Inscriptiones saracenicas continens, Paris, 1950; Plates, 1951. - ClAS : V.A. Clark, A Study of New Safaitic Inscriptions from Jordan, Melbourne, 1979 (UMI, Ann Arbor, MI, 1983).

DAFA : R. Blachère et al., Dictionnaire arabe-français-anglais (Langue classique arabe et moderne), Paris, I (1967) and II (1976).

Eut : J. Euting; for the edition of Euting's copies of Lih, Min and Tham texts, cf. JaMAR, V.

FrLAL : G.W. Freytag, Lexicon arabico-latinum, 4 vol., Halles, 1830 - 1837.

Gl : E. Glaser.

HaAIC : G.L. Harding, An Index and Concordance of the Pre-Islamic Names and Inscriptions, Toronto, 1971. - HaLi : texts published in HaLiSTI. - Ḥaḍr : Ḥaḍrami. - HaLiSTI : G.L. Harding and E. Littmann, Some Thamudic Inscriptions from the Hashemite Kingdom of Jordan, Leiden, 1952. - Hu : texts published by C. Huber in Journal de voyage en Arabie (1883-1884), Paris, 1891. - HuIR : Tham texts published by C. Huber in Bulletin de la société de géographie, 5 (1884), pp. 289-303.

Ja : SA texts published by A. Jamme. - JaCIASA : A. Jamme, "Corpus des inscriptions et antiquités sud-arabes," in Orientalia, 48 (1879), pp. 260-284. - JaCMYE: A. Jamme, Carnegie Museum [illegible] Yemen Expedition, [illegible]. - A. Jamme, "Inscriptions du Musée de Ṣan'â' d'après les photographies de M.C. Conti Rossini," in Le Muséon, 67 (1954), pp. 323-338. - JaL: Lih texts published by A. Jamme. - JaMAR : A. Jamme, Miscellanées d'ancient arabe, Washington, I (1971), II (1971), III (1972), IV (1973), V (1974), VI (1974), VII (1974), VIII (1979), IX (1979), XII (1982), XIII (1983), XIV (1985), XV (1987). - JaMIL : A. Jamme, Minaean Inscriptions Published as Lihyanite, Washington, 1968. - JaP : A. Jamme, "Le panthéon sud-arabe préislamique d'après les sources épigraphiques," in Le Muséon, 60 (1947), pp. 57-147. - JaPRER : A. Jamme, "Preliminary Report on Epigraphic Research in Northwestern Wâdî Ḥaḍramawt and at al-'Abr," in BASOR, No. 172, December, 1963, pp. 41-54. - JaS : Saf texts published by A. Jamme. - JaSaMA : J.A. Jaussen et M.R. Savignac, Mission archéologique en Arabie (mars-avril 1907), Paris, II, 1914. - JaSIMB : A. Jamme, Sabaean Inscriptions from Maḥram Bilqîs (Mârib), Baltimore, 1962. - JaSN : A. Jamme, Safaitic Notes (Commentary on JaS 41-176), Washington, 1970. - JaT : Tham texts published by A. Jamme. - JaTS : A. Jamme, Thamudic Studies, Washington, 1967. - JsaL : Jaussen-Savignac, Lih. - JsaT : Jaussen-Savignac, Tham. - JoAES : W.J. Jobling, "The 1982 Archaeological and Epigraphic Survey of the 'Aqaba-Ma'an Area of Southern Jordan," in ADAJ, 27 (1983), pp. 185-196 and pl. 32-38. - JoAM : W.J. Jobling, "The Aqaba [sic!]-Ma'an Archaeological and Epigraphic Survey 1982 Season," in Liber annuus, 32 (1982), pp. 467-470 and pl. 111-113. - JoAMA, II : W. J. Jobling, "The 'Aqaba-Ma'an Archaeological and Epigraphic Survey 1980-1984. Some Notes and Reflections," in TRASUS, 2 (1984), pp. 34-52. - JoAMA, III : W.J. Jobling, idem, 1983-1985," in AfO, 33 (1986), pp. 233A-247B. - JoAMS : W.J. Jobling, "Aqaba [sic!]-Ma'an Survey, Jan.-Feb. 1981," in ADAJ, 26 (1986), pp. 199-209 and pl. in pp. 479-481. - JoDD : W.J. Jobling, "Desert Deities: Some New Epigraphic Evidence for the Deities Dushares and al-Lat from the Aqaba [sic!]-Ma'an Area of Southern Jordan," in Religious Traditions, 7-9 (1984-1986), pp. 25-40. - JoFS : W.J. Jobling, "The Fifth Season of the 'Aqaba [sic!] Ma'an Survey 1984," in ADAJ, 28 (1984), pp. 191-202 and pl. in pp. 468-472. - JoFSA : W. J. Jobling, "The Fifth Season of the 'Aqaba-Ma'an Archaeological and Epigraphic Survey, 1984," in Liber annuus, 34 (1984), pp. 423-428 and pl. 85-86. - JoNAT, I : W.J. Jobling, "North Arabian (-Thamudic) Inscriptions and Rock Art from the 'Aqaba-Ma'an Area of Southern Jordan. Selection I," in ADAJ, 30 (1986), pp. 261-267, facsimiles in pp. 268-283, and pl. in pp. 478-485. - JoPRF : W.J. Jobling, "Preliminary Report on the Fourth Season of the 'Aqaba-Ma'an [sic!] Archaeolo-

gical and Epigraphic Survey, 1982/1983," in ADAJ, 27 (1983), pp. 197-208 and pl. 39-49. - JoPRS : W.J. Jobling, "Preliminary Report on the Sixth Season of the ʿAqaba-Haʿan Epigraphic and Archaeological Survey," in ADAJ, 29 (1985), pp. 211-219 and pl. in pp. 341-348. - JoMoPSA : W.J. Jobling and R.T.H. Morgan, "The Fourth Season of the ʿAqaba-Haʿan Archaeological and Epigraphic Survey 1982-1983," in Liber annuus, 33 (1983), pp. 396-401 and pl. 67-69.

LaAEL : E.W. Lane, Arabic-English Lexicon, 8 vol. - LaGD : Landberg, Glossaire datinois, Leiden, I (1920) and II (1923). - Lih : Lihyanite. - LiNAD : E. Littmann, "Neues zur altarabischen Dialektkunde," in ZDMG, 99 (1950), pp. 168-180. - LP : Saf texts published in E. Littmann, Safaitic Inscriptions, Leiden, 1943.

Min : Minaean.

NA : North-Arabian.

Qat : Qatabanian.

RÉS : Répertoire d'épigraphie sud-sémitique, Paris, VII (1950). - RoAAH : F. Rosenthal, An Aramaic Handbook, Part 1/2, Wiesbaden, 1967. - RoHT : C. Robin, Les hautes-terres du Nord-Yémen avant l'Islam, Istanbul, 1982. - RoPH : C. Robin, Le pays de Hamdān et Ḥawlān Quḍāʿa (Nord-Yémen) avant l'Islam, Paris, 1977.

SaAAL : H.W. Salmone, An Advanced Learner's Arabic-English Dictionary, Beirut, 1972. - Sab : Sabaean. - SDDS : A.F.L. Beeston - M.A. Ghul - W.W. Müller - J.Ryckmans, Sabaic Dictionary - Dictionnaire sabéen - al-muʿjam as-sabaʾay, Louvain, 1982.

WH : Saf texts published in WiHaI. - WiHaI : F.V. Winnett and G.L. Harding, Inscriptions from Fifty Safaitic Cairns, Toronto, 1978. - WiReAR : F.V. Winnett and W.L. Reed, Ancient Records from North Arabia, Toronto, 1970. - WiSLTI : F.V.Winnett, A Study of the Lihyanite and Thamudic Inscriptions, Toronto, 1937.

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Addendum to VII (pp. 135-159): Professor N. Lapp on the Bethel stamp.

In an effort to gather all possible information on the unearthing of the Bethel stamp by James L. Kelso, I requested Professor N. Lapp to kindly check the excavation report. I wish to express my deep gratitude to Professor N. Lapp for her gracious collaboration and her kindness in sending to me the result of her investigation in her letter of last October 12, 1988, which states the following:

> "The most I can come up with is from the registration book. It states that the seal was from the West Gate, 190 cm. deep.
> Sometime I may be able to do further study, but I am rather doubtful that it will be possible to come up with much more information. The method of excavation and record keeping just do not provide more help."

Kelso's testimony remains intact (cf. above, p. 158); consequently, the seal was found "well [= 190 cm] below the undisturbed surface-level above it." This information excludes the hypothesis of any plant which would have left the easily recognizable traces of a trench and of a disturbed soil.